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NINTH MEETING OF THE BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

[From our own Correspondents.]

SATURDAY, AUGUST 24.

Many circumstances combined to throw a gloom over the opening of the Birmingham session of the British Association. The recent disturbances had rendered it doubtful whether the meeting might not be adjourned to another time and place; foreigners were alarmed by the exaggerated accounts of the riots; and the members of the Local Committee, having their attention engaged by the exciting events around them, could not bestow much of their time on preparations for the reception of the Association. Indeed, under circumstances, it was surprising that their arrangements were so perfect as we found them. Many members, however, were disappointed at the very limited number of manufactories opened to general inspection; and it was thought to be most strange, and was a subject of general remark, that no one of the great engineering establishments, for which Birmingham is so renowned, were found in the list. The Local Committee, however, and all who aided them, deserve great credit for their exertions in collecting together the 'Illustrations of Manufactures, Inventions, and Models, &c.' exhibited at the Grammar School, and which did infinite credit to the town, and should, we submit, serve as the basis of a permanent collection of a similar character. All the specimens were interesting or instructive, and many beautiful as works of art.

GENERAL COMMITTEE.

The Committee met in the Library of King Edward's School at one o'clock, and adjourned to three, in order to give time for the arrival of members expected by the afternoon trains; for, strange as it may appear, the very facilities of travelling afforded by the rail-roads tended to reduce the numbers present at this early period. On other occasions, the obvious limits to the means of conveyance induced many persons to start early, and thus avoid the crowd; but as no such precaution was now necessary, their journey was deferred to the latest possible moment. At three o'clock the chair was taken by the Rev. Vernon Harcourt, the President of the Association; and Mr. Yates read a report of the proceedings of the Council during the past year. It principally related to the preliminary arrangements made with the Local Committee at Birmingham. The finances of the Association were stated to be as follows:

RECEIPTS.

Balance in hand	£	670	0 7
Compositions from Life Members		641	0 0
Subscriptions for 1838		1944	0 0
Do. 1839		37	0 0
Arrears		37	1 0
Sale of Reports		412	9 6
Dividend on Stock		165	0 0
	£	3906	11 1

DISBURSEMENTS.

Expenses at Newcastle	£	500	0 0
Paid to Local Treasurers		156	18 5
Salaries, &c.		250	0 0
Grants to Committees for scientific purposes:			
For Reduction of Stars in Histoire Celeste	£321	18	6
Do. Lacaille	11	0	0
Catalogue of Stars ..... 1837..	6	16	6
Land and Sea Level ..... 1838..	52	1	4
Do. ..... 1837..	222	0	0
Tide Discussion at Bristol	25	18	6
Mechanism of Waves ..... 1837..	94	2	0
Do. ..... 1838..	50	0	0
Meteorolog. Observations, Plymouth	55	0	0
Do. do. hourly, Scotland	49	7	8
Completing Anerdometer	8	10	0
Meteorology and Subterran. Temp. 1837, Thermometer	21	11	0
Atmospheric Air	16	1	0
Action of Sea Water on Iron	40	0	0
Action of hot Water on Organic Bodies	3	0	0
Bristol Fossil Ichthyology	110	0	0
Fossil Reptiles	118	2	9
Mining Statistics	50	0	0
Duty of Cornish Steam-engines	50	0	0
Marine Steam-engines	100	0	0
Experiments on Vitrification, *old grants*	9	4	7
Do. on Strength of Iron	100	0	0
Animal Secretions ..... 1837..	10	13	0
Gases on Solar Spectrum, Action of	22	0	0
Railway Constants.. 1837.. £8 7 2			
Do. ..... 1838 .... 20 0 0	28	7	2
	1595	11	0
Carried forward	£2502	9	5
Brought up	£2502	9	5
Printing Reports	301	14	0
Do. Lists of Members	77	12	0
Advertising &c.	38	14	6
Sundries	25	7	10
Balance in hand	460	13	4
	£3906	11	1

On the motion of the Marquis of Northampton, seconded by R. Hutton, Esq. M.P., Mr. Francis Baily was elected a trustee of the Association, in the room of Mr. Babbage, resigned.

The following Rules, recommended for adoption by the Council, were unanimously agreed to:

1. That an admission fee of 1*l.* be required from all Members admitted as Annual Subscribers at future Meetings, in addition to their Annual Subscription of 1*l.*; the composition of Life Members to remain at 5*l.*

2. That every Member, new and old alike, have the option of paying 5*l.* as a fixed *Book-subscription*, entitling him to receive a copy of the volumes published after the date of such subscription.

3. That all recommendations of Grants of Money, requests for special Researches, and Reports on scientific subjects, be submitted to the Committee of Recommendations, and not taken into consideration by the General Committee, unless previously recommended by the Committee of Recommendations.

Dr. Yelloly withdrew the requisition for the establishment of an Agricultural Section, on the understanding that the subject would be taken into consideration at the next meeting of the Association; and Mr. Kingsley withdrew a series of resolutions respecting the arrangement of business, and agreed to submit them to the Council.

The following were chosen to form the Committee of Recommendations:—Dr. Buckland, Dr. Daubeny, Prof. Forbes, Prof. Graham, Mr. Gray, Mr. Greenough, Mr. Hallam, Dr. Hodgkin, Mr. Hutton, Prof. Lloyd, Mr. Lyell, Marquis of Northampton, Mr. Porter, Dr. Roget, Mr. Fox Talbot, Prof. Wheatstone, Prof. Whewell, Mr. Willis, Dr. Yelloly.

The Officers and Committees of Sections were chosen as usual; their names will be found prefixed to the several reports.

The Committee then adjourned to Thursday, the 29th, at three o'clock.

MONDAY, AUGUST 26.

SECTION A.—MATHEMATICAL AND PHYSICAL SCIENCE.

President—Rev. Professor Whewell.
Vice-Presidents—Mr. Francis Baily, Prof. Forbes, Major Sabine.
Secretaries—Messrs. J. T. Chance, W. Snow Harris, Prof. Stevelly.
Committee—Rev. J. Abbott, The Dean of Ely, Rev. Sydney Gedge, Mr. E. Hodgkinson, Rev. H. Holditch, Mr. W. Hopkins, Capt. Johnson, R.N., Rev. Prof. Lloyd, Rev. W. Lawson, Rev. Dr. Lardner, Mr. F. Osler, Prof. Phillips, *Assist. Gen. Sec.*, Le Comte de Pambour, Rev. Prof. Powell, Messrs. J. S. Russell, J. Ryall, W. H. Fox Talbot, Prof. Wheatstone, Prof. Willis, Hon. J. Wrottesley.

The President briefly addressed the Section. He observed, that one of the chief objects of the Association was, to grant sums of money to individuals or committees engaged in the pursuit of particular branches of science, and it was a very proper regulation, that the Reports of the manner in which these sums had been expended, whether in whole or in part, should be brought before the Section at whose instance they were granted. And even in cases where the object had not been attained, or the money had not been expended, that a Report must be given in, and the application for the grant renewed; otherwise it was considered to have lapsed. It, therefore, now became his duty to call on the Secretary to read certain Reports; two of them from Sir John Herschel, and one from Sir David Brewster, gentlemen whose absence from the present meeting, he felt sure, the Section would unite with him in deploring.

The Secretary then read the reports.

On the subject of a resolution agreed to by the British Association, at their meeting at Newcastle, in August, 1838, to the following effect:—"Resolved —That it is desirable that the whole of the stars observed by Lacaille at the Cape of Good Hope, the observations of which are recorded in his Cœlum Australe Stelliferum, should be reduced. That Sir J. Herschel, Mr. Airy, and Mr. Henderson, be a committee for carrying the same into effect, and that 200*l.* be appropriated for the purpose,"—

The Committee report—That considerable progress has been made in the reduction of the stars in Lacaille's Cœlum Australe Stelliferum; and that, although only a small portion of the money appropriated, has been actually expended, nearly the whole will probably be required, during the ensuing year, to complete the work.

August 17, 1839. J. F. W. Herschel.

On the subject of a resolution adopted by the meeting of the British Association, held at Newcastle, to the following effect:—"Resolved —That it is desirable that a revision of the nomenclature of the stars should be made, with a view to ascertain whether or not a more correct distribution of them amongst the present constellations, or such other constellations as it may be considered desirable to adopt, may be formed. That Sir J. Herschel, Prof. Whewell, and Mr. Baily, be a committee for that purpose, and do report on the same, at the next meeting of the Association. That the sum of 50*l.* be appropriated to defray any expenses that may be incurred in this inquiry,"—

Your Committee report—That some progress has been made in reforming the nomenclature of the Northern Constellations; and that the stars in the Southern have been commenced laying down on a planisphere according to their observed actual magnitudes, for the purpose of grouping them in a more convenient and advantageons manner. No expense has yet been incurred in this inquiry; but the Committee are desirous that the grant should be continued for another year.

August 17, 1839. J. F. W. Herschel.

On a grant of 500*l.* made for the reduction of stars in the *Histoire Céleste*, under the superintendence of Mr. Baily, Mr. Airy, and Dr. Robinson,—

Report—The reduction of the stars in the *Histoire Céleste* has been commenced, and already about 13,000 stars have been reduced, at an expense of about 170*l.* It is presumed, that the greater part (if not the whole) of the remainder may be completed in the course of the ensuing year; and it is, therefore, expedient that the grant of money should be continued.

August 20, 1839. Francis Baily.

On a grant of 500*l.* made for the purpose of extending the catalogue of stars of the Royal Astronomical Society, under the direction of Mr. Baily, Mr. Airy, and Dr. Robinson,—

Report—With respect to the extension of the Astronomical Society's catalogue of stars, about one-half of the computations are completed, in consequence of the liberal grant from the British Association at Liverpool; and about 180*l.* of that grant has been expended. The whole of the remainder of the grant will probably be required before the next meeting of the Association.

August 20, 1839. Francis Baily.

The President made a few remarks on the advantage which would result to the science of Astronomy, so soon as the several objects proposed in the reports just read, should be obtained, and inquired whether Mr. Baily wished to make any additional observations to the Section. Mr. Baily said, that he did not think it necessary at present to go farther into detail; any person acquainted with astronomical subjects, would be aware how entirely useless the vast mass of observations now in progress of being reduced was, without that reduction. The places of comparatively few of the fixed stars were at present known to astronomers, with the precision which modern science demanded, although the materials

for determining those places had been for years uselessly locked up in these vast and rich storehouses, which the British Association were now in progress of having unlocked.

'On the best positions of three Magnets, in reference to their mutual action,' by the Rev. H. Lloyd.—It is a problem of much importance, in connexion with the arrangement of a magnetical observatory, to determine the relative position of the magnetical instruments in such a manner, that their mutual action may be either absolutely null, or at least readily calculable. Such was stated by the author to be the object of the present investigation. In the case of *two* horizontal magnets, one of which (intended for observations of *declination*) is in the magnetic meridian, and the other (used for observations of *horizontal intensity*) is in the perpendicular plane, there is nothing to compensate the action of each magnet on the other. The best thing that can be done in this case, is to determine the position of the second magnet in such a manner, that the direction of its action on the first shall coincide with *the magnetic meridian*. In such case, the position of the first magnet will be undisturbed by the second, so as to give the *absolute declination* truly; and, as to the *variations* of the declination, it is manifest that they will be thereby increased or diminished in a *given ratio;* so that the true variations will be obtained by simply multiplying by a constant coefficient. In this case, however, the action of the first magnet on the second will not take place, either in the magnetic meridian, or in the plane perpendicular to it; so that the second magnet is necessarily disturbed. The case is different when a *third* magnet is introduced. It is then possible to annul completely all action, with the exception of that exerted on the third magnet by the first and second; and this, in the case under consideration, is destroyed by the nature of the suspension. The third magnet employed in the Dublin Observatory is intended for the observation of the *vertical component of the magnetic force*. It is a bar supported on knife-edges, capable of motion in a vertical plane, and brought into the horizontal position by means of a weight. The three instruments being in the same horizontal plane, it is manifest that the action of the first and second on the third must take place in that plane; and this action can have no effect in disturbing the magnet, its motion being confined to the vertical plane. It is only necessary to consider, therefore, the action on the first and second magnets. Mr. Lloyd then stated the conditions of equilibrium of these actions, which were expressed by four equations, containing four arbitrary angles; so that this equilibrium is, in general, attainable, by suitably determining the position of the three magnets, whatever be their relative intensities. In practice, however, it will seldom happen, that we can regard as arbitrary all the four angles which enter into these equations, one or more of them being, in general, determined by some circumstance connected with the locality. In such case the complete destruction of all action is no longer possible; and we must look for some other solution of the problem of mutual interference. Next to the complete destruction of all action, the most desirable course is to give to the resultant action such a direction, that its effect may be readily computed and allowed for. In the case of the declination bar, it is easily seen that this direction is the magnetic meridian itself; the *mean* position of the bar being thereby *unaltered*, and the *variations* of its position only increased or diminished in a given ratio. By means of a simple investigation it may be shown, that the same thing is true of the horizontal intensity bar; and that, in order that the variations of *declination* may not be mixed up with those of *force*, the resultant force exerted upon this magnet by the other two must lie in the magnetic meridian. The problem, therefore, is reduced to this:—to determine the position of the three magnets A, B, and C, in such a manner, that the resultant actions exerted upon A and B, respectively, by the other two, shall lie in the magnetic meridian. The solution of this problem was shown by Mr. Lloyd to be contained in two equations, which may, of course, be satisfied by means of two unknown angles; so that, when we have a greater number of undetermined quantities, some of them remain arbitrary, and the conditions may be fulfilled in various ways. Mr. Lloyd concluded, by adverting to some of the more remarkable of these solutions, as when the three magnets are in the same right line, &c.

In reply to a question from the President, Mr. Lloyd briefly explained to the Section the arrangement of the *portable* observatory, adopted by Capt. J. Ross, in his preparations for the Antarctic expedition. It is so constructed, as to form, either *three* small separate rooms, or *one* large one. The former arrangement being desirable at places where the dip is nearly 90°, and where, consequently, the horizontal direction force is very small, and the disturbing action of the magnets on one another, relatively great. The parts are put together with copper fastenings; and the whole is so arranged, as to occupy a very small bulk when in pieces, and to be capable of being put together with quickness and security.

Mr. Byrne inquired whether the instruments themselves furnished the measure of the intensity of action which was referred to in the formula.—Prof. Lloyd replied that the measure of intensity was to be had by multiplied experiments; but when once obtained with sufficient accuracy, it furnished a constant coefficient for each instrument.

Sir David Brewster's 'Report respecting the two series of hourly Meteorological Observations kept in Scotland,' was next read by Prof. Forbes:—

"Having fixed upon Inverness and Kingussie as two suitable stations for carrying on the two series of hourly observations, which I undertook to establish and superintend for the British Association, I was fortunate in being able to prevail upon the Rev. Mr. Rutherford, of Kingussie, and Mr. Thomas Mackenzie, teacher of Raining's School, Inverness, to carry on these observations. The instruments which were necessary for this purpose, were made by Mr. Adie, of Edinburgh, under the superintendence of Prof. Forbes, and the observations commenced on the 1st of November, 1838, the beginning of the meteorological year, or the first of the group of winter months. I directed the two observers to pay particular attention to the Aurora Borealis, and to record every phenomenon of this nature; and I have no doubt, from the lists already sent me, that this class of observations will be the most complete and valuable that have ever been made. I annex a specimen of the observations made at Kingussie at the time of the great depression of the barometer, on the 29th of November, 1838, which will exhibit the nature and value of the register. As it is of the greatest importance to obtain the true curve of the daily variation of temperature and pressure at the two stations of Inverness and Kingussie, the last of which places is between 700 and 800 feet above the level of the sea, I earnestly hope that the Association will permit these observations to be carried on for at least another year.

"St. Leonards, St. Andrews, DAVID BREWSTER."
"August 22, 1839."

The Secretary stated, that a sheet accompanied this report, containing an extract from the register of observations of the barometer and thermometer kept at Kingussie by A. Rutherford; this comprised tables of the hourly observations of the barometer and thermometer during the 27th, 28th, 29th, and part of the 30th of November, 1838, the barometer having gradually sunk down to the lowest 27.200 on the 29th, at 3 P.M., and risen again rapidly to its usual height after 9 A.M. of the 30th. There were also some notes respecting the Aurora, which the President directed to be read, and which were as follows:—

"13th of November, 2 o'clock, A.M., aurora till 6 o'clock, A.M. The following are the more minute details on this, and the only time worth noting. Having been awakened by the assistant at a quarter to 2, A.M., I found a rather bright aurora, consisting of an arched band, from N.E. to N.W., with a few streams of light darting up to the zenith; there were also a few light clouds floating along the northern horizon, with a haze of light on the background. At 3, A.M., streaming up very bright—and as formerly—light clouds over the aurora. At a quarter past 3, A.M., very bright. Besides the arched band formerly mentioned, there was another dimmer arch higher up, extending from a little to the north of east, quite across the sky to a little north of west; its upper border reaching the seven stars, or Pleiades. Between these two arches, there were a great many spires of light darting up, and recoiling, with frequently a sort of waving flash running over the slightly illumined parts, and even where no spires of light were visible, except where the flash gave them a momentary existence. Clouds on the horizon, but not many. Twenty-five minutes to 4, A.M., a few clouds on the horizon, a bright arch above them, but few spires of light. Quarter to 4, A.M., as last noted, but streaming more up to the zenith. 4 o'clock A.M., a few clouds on the north-west horizon—the light dim, few spires, and those extending only a short way up the sky. Quarter past 4, A.M., as last noted, with an arch of light a little above the horizon, right over north, and spires of light darting up. Half past 4, A.M., ditto, ditto. Quarter to 5, A.M., few clouds, but a dark colour below the arch near the horizon. 5, A.M., clear coruscations flashing up to the zenith, the upper edge or arch passing through it, or nearly so, from north-east to about due west, touching Orion's belt with its upper border, flashes of light rolling from west to east, illuminating lines of the aurora perpendicular to the lower arch, and reaching the higher one, but only rendered visible by the coruscations passing over them. The aurora brightest where there are clouds on the horizon. No wind or frost, although clear, except the few clouds mentioned. Half past 5, A.M., coruscations from the horizon up to the zenith, in a hobbling manner. 6 o'clock, only a dim light. 17th of November, 1838, 10 o'clock, P.M., slight aurora, but not visible at 11, P.M. 14th of December, 1838, 9, P.M., slight aurora, tops of the spires only visible above the horizon in the north. 11, P.M., only a dim light."

The Secretary further stated, that it was noted opposite the hour of the great depression of the barometer on the 29th of November, "that from the 27th to the 30th, only half an inch of rain fell, notwithstanding the low state of the barometer. The depression seems to have been more the result of high gusty winds, and probably falls of rain, or other commotions in the neighbourhood, than any immediate fall here."

Prof. Stevelly observed that the depression stated to have taken place at Kingussie, was very nearly the same as the lowest to which the barometer fell on that occasion at Belfast.—Prof. Forbes asked, If that were on the level of the sea? Being answered yes, he remarked, that the observations at Kingussie were made at an elevation of from 700 to 800 feet above the sea. At Edinburgh the depression was 27.777.—Lieut. Morison said, the depression which he had noted, was much below that stated.—Prof. Stevelly suggested that there might have been a slight portion of air in the barometer tube.—Mr. Whewell mentioned, that he had lately seen a very curious picture in a cabinet of curiosities of a friend at Utrecht; it was by a Japanese artist, and represented a South Polar whale-fishing scene. The line of the horizon was very distinctly marked, and above it was placed an arch of light, obviously designed to represent an aurora, but, most curiously, the coruscations, or what Mr. Rutherford in the notes just read calls the spires, radiated from the centre of the arch, and not towards the zenith. This, he supposed, if not altogether a mistake of the artist, must have arisen from some unintelligible error in the perspective.—Prof. Stevelly said, that in the remarkable red aurora of, he believed the 5th of February, 1838, an oval space was illuminated round the zenith, and lines of auroral light, extended, of course, apparently in a diverging direction from it to the horizon, on all sides south of east and west. Now, if this aurora were seen in a high latitude, where part of that oval space was below the horizon, and the rest and the lines of light above, the appearance described by the President would be exactly presented to the spectator.—Prof. Lloyd remarked, that this was one portion of Dalton's theory of the aurora.

The Secretary read a letter from Prof. Bache, of Philadelphia, "Respecting a report which the British Association, assembled at Newcastle, had requested him to prepare, on the state of Meteorological Science in America."

Philadelphia, July 29, 1839.

EXCUSE me for writing you a letter which must reach you just as you are beginning your duties at Birmingham, if it have good speed, and which, on that account, I will cut as short as possible. I pray you to make to the Committee of Section A, or the other appropriate committee, my excuses for not complying with their request, in regard to a report on the state of Meteorology here, for this meeting. I will try to send them over for the next; but as there is a *tangle*

in the state, which I must undo, it is better done deliberately. Various circumstances, since my return, have put the completion of this report out of the question; my health has been bad, my time much taxed for the Giraud College in drawing up a report of my tour on your side of the water, and in endeavours to get the machine in motion, and I have had no fixed "local habitation," and, therefore, have wanted some of the appliances of general study.

I must write to you more at length about rain. My gauges were at first as high above all parapets as your Museum gauge, and worked *so* irregularly. About the ground I am less sure, but think it worth looking into. Even my high poles have not cured, in all cases, the defect at the top of the tower, as appears by a series kept for me during the first year of my absence. I will join you most heartily in any scheme to decide your knotty question, but expect a full solution from your own efforts.

The magnetic arrangements are admirable. I trust to be able to work in concert, and am pressing for an observatory. The instruments, except Lloyd's vertical force one, which is ordered, I have already. A. D. Bache.

In reference to the little reliance which could be placed on the indications of rain gauges in elevated stations, Prof. Stevelly directed the attention of Prof. Forbes, who was not at the last meeting, to the suggestion then made by Dr. Robinson, of Armagh, (see *Athen.*, No. 566,) of a remedy for the evil, by a globular ball, which would present the same circular section to the rain, come in what direction it might; the water being then conveyed, by capillary attraction and gravity, along the surface of the ball to a tube placed below. He then showed, by a diagram, how different parts of the same shower, falling with constant regularity, but subject to currents of air in several directions, might deposit very different amounts of water in differently situated gauges at the same time.—Prof. Lloyd said that he had tried some experiments on this subject, and that he unquestionably found that there were discrepancies, but by no means so great as he had feared; and he thought a gauge would afford a pretty fair indication of the average quantity that fell on the horizontal surface.—Prof. Forbes said that there were unquestionably great difficulties in the way of coming to satisfactory conclusions from the indications of our ordinary rain-gauges, and that all that could at present be said for them was, that they afforded pretty fair average results. As to the suggested globe of Dr. Robinson, he saw many objections to it: indeed, he scarcely knew how it could be applied in practice, for, if the surface were smooth, almost all the drops that fell on it would rebound; and, if it were rough, so as to retain them, the evaporation would be so large as entirely to derange the conclusion from its indications, unless a most troublesome and unsatisfactory correction were applied after a lengthened series of observations.—The President said that the indications of the rain gauge were most important to the meteorologist, but, unfortunately, the instrument, in its present form, was very defective; still he thought its indications were not to be despised, as leading to average and relative conclusions.

Prof. Daubeny now exhibited the model of an apparatus, by means of which, in a more complete condition, he hoped to obtain a numerical estimate of the intensity of solar light at different periods of the day, and in different parts of the globe. The contrivance consisted of a sheet of photogenic paper, moderately sensible, rolled round a cylinder, which, by means of machinery, would uncoil at a given rate, so as to expose to the direct action of the solar rays, for the space of an hour, a strip of the whole length of the sheet, and of about an inch in diameter. Between the paper and the light was to be interposed a vessel, with plane surfaces of glass at top and bottom, and in breadth corresponding to that of the strip of paper presented. This vessel, being wedge-shaped, was fitted to contain a body of fluid of gradually increasing thickness, so that, if calculated to absorb light, the proportion intercepted would augment in a gradually increasing proportion from one extremity of the vessel to the other. Hence it was presumed that the discoloration arising from the action of light would proceed along the surface of the paper to a greater or less extent, accordingly as the intensity of the sun's light was such as enabled it to penetrate through a greater or lesser thickness of the fluid employed. In order to register the results, nothing more was required than to measure, each evening, by means of a scale, how many degrees the discoloration had proceeded along the surface of the paper exposed to light, during each successive hour of the preceding day. To render the instrument self-registering, some contrivance for placing the paper always in a similar position with reference to the sun, must of course be superadded. The object of this contrivance differed from that aimed at by Sir J. Herschel in his Actinometer, being intended as a measure of the aggregate effect of the solar intensity at the period (be it long or short) during which the paper was submitted to its influence; whereas the Actinometer merely measures the intensity at the moment the observation is made. The interposition of an absorbing fluid has at least this advantage, that it enables the observer to estimate the relative intensity by marking the point at which the paper ceases to be discoloured, of which the eye is able to judge more exactly than it could do of the relative darkness of shade which might be produced on paper exposed unprotected to light of different degrees of brilliancy.

Mr. Jackson thought that a Heliostat, for throwing the reflected light of the sun upon the instrument, would be objectionable; and suggested, in preference, that the Heliostat should rather turn the instrument to the sun, which he considered could be effected with equal ease.—Dr. Daubeny assented.—Prof. Forbes only first saw the instrument yesterday; he therefore could not speak with much confidence, but must own his first impressions were not very sanguine of its ultimate success. One objection was, the difficulty, if not impossibility, of procuring prepared or photogenic paper of exactly similar sensibility; and unless this were done, not even the observations of the same person, made at different times, could be compared, much less could any comparisons be made of the results of different observers. Another objection was the difficulty of observing where the discoloration extended to, as the shading off would be so gradual; so that different persons would come to different conclusions, according to the goodness or badness of their sight, upon the very same sheet of register paper. A third objection was, that the scale would be so complicated, that he much feared whether any scale could be formed; for not only were the quantities of light which would be permitted to pass through a wedge-shaped mass of fluid, such as Dr. Daubeny proposed, diminished, in a geometrical proportion, as its thickness increased arithmetically, but it had now been clearly established by the researches of Melloni, Fresnel, and others, that the portion of light which had already passed through a portion of any medium, was in a state for passing with greater ease, or in a greater relative proportion, through equal portions of the remainder. Unquestionably self-registering instruments of all kinds were highly desirable, and an instrument fitted to perform what Dr. Daubeny proposed would be both interesting and valuable; and he thought the suggestions valuable, even supposing difficulties such as he alluded to should be found to exist.—Dr. Daubeny said, that, as to one of the objections, Sir John Herschel had informed him that any quantity of equally sensible photogenic paper could be obtained; as to the scale, the indications were not intended to furnish absolute, but only relative results.

Mr. Talbot then rose to offer a few remarks on M. Daguerre's photogenic process. M. Arago had stated to the Institute that the sciences of Optics and Chemistry united were insufficient in their present state to give any plausible explanation of this delicate and complicated process. If M. Arago, who had had the advantage of being for six months acquainted with the secret, and therefore of considering its nature in all points of view, was of this opinion, it seemed as if a call were made on all the cultivators of science to use their united endeavours, by the accumulation of new facts and arguments, to penetrate into the real nature of these mysterious phenomena. For this reason, Mr. Talbot said, he would offer to the Section a small contribution, on his part, of new observations which might perhaps be of service towards the elucidation of this new branch of science. The first part of M. Daguerre's process consists in exposing a silver plate to the vapour of iodine, by which it becomes covered with a stratum of iodide of silver, which is sensitive to light. Mr. Talbot stated that this fact had been known to him for some time, and that it formed the basis of one of the most curious of optical phenomena, which, as it did not appear to have been observed by M. Daguerre, he would describe to the meeting. Place a small particle of iodine, the size of a pin's head, on a plate of silver, or on a piece of silver leaf spread on glass. Warm it very gently, and you will shortly see the particle become surrounded with a number of coloured rings, whose tints resemble those of Newton's rings. Now, if these coloured rings are brought into the light, a most singular phenomenon takes place; for the rings prove to be sensitive to the light, and their colours change, and after the lapse of a short time their original appearance is quite gone, and a new set of colours have arisen to occupy their places. These new colours are altogether unusual ones; they do not resemble anything in Newton's scale, but seem to conform to a system of their own. For instance, the two first colours are, *deep olive green*, and *deep blue inclining to black*, which is quite unlike the commencement of Newton's scale. It will be understood that the outermost ring is here accounted the first, being due to the thinnest stratum of iodide of silver, farthest from the central particle. The number of rings visible is sometimes considerable. In the centre of all, the silver leaf becomes white and semi-transparent, like ivory. This white spot, when heated, turns yellow, again recovering its whiteness when cold: from which it is inferred to consist of iodide of silver in a perfect state. The coloured rings seem to consist of iodide of silver in various stages of developement. They have a further singular property, which, however, has not been sufficiently examined into. It is as follows: It is well known that gold leaf is transparent, transmitting a bluish green light; but no other metal has been described as possessing coloured transparency. These rings of iodide of silver, however, possess it, being slightly transparent, and transmitting light of different colours. In order to see this, a small portion of the film should be isolated, which is best done by viewing it through a microscope. Mr. Talbot said, that he had considered the possibility of applying a silver plate thus combined with iodine to the purpose of photogenic drawing, but he had laid it aside as insufficient for that purpose, on account of its sensitiveness appearing to be much inferior to that of paper spread with chloride of silver, and therefore in an equal time it takes a much feebler impression. Now, however, M. Daguerre has disclosed the remarkable fact, that this feeble impression can be increased, brought out, and strengthened, at a subsequent time, by exposing the plate to the vapour of mercury. Another experiment was then related, in which a particle of iodine was caused to diffuse its vapour over a surface of mercury. In order to this, a copper-plate was spread over with nitrate of mercury, and then rubbed very bright, and placed in a closed box along with a small cup containing iodine. The result was, a formation of Newton's rings of the greatest splendour, and of a large size. But they did not appear to be in any degree sensitive to light. The next point of M. Daguerre's process is, the exposure of the picture to the vapour of mercury—and this is by far the most enigmatical part of the whole process. For, he states that if you wish to view the picture in the usual manner, that is, vertically, you must hold the plate inclined to the vapour at an angle of 45°, and *vice versâ*. Now this is something altogether extraordinary; for who ever heard of masses of vapour possessing determinate *sides*, so as to be capable of being presented to an object at a given angle? From the hasty consideration which he had been able as yet to give to it, his first impression was, that this fact bore a certain analogy to some others which he would mention. If a piece of silver leaf is exposed to the vapour of iodine, however uniform the tension of the vapour may be, yet it does not combine uniformly with the metal, but the combination *commences* at the edge of the leaf and spreads inwards, as is manifested by the formation of successive bands of colour parallel to the edge. This is not peculiar to silver and iodine, but occurs when other metals are exposed to other vapours: not always with entire regularity, but it displays a tendency to combine in that way. A possible explanation is, that this is due to the powerful electrical effect which the sharp edges and points of bodies are known to possess; in fact, that electricity is either the cause or the attending consequence of the combination of vapour with a metallic body. Again, if a minute particle of iodine is laid on a steel plate, it liquefies, forming an iodide of iron, and a dew spreads around the central point. Now, if this dew is examined in a good microscope, its globules are seen not to be

arranged casually, but in straight lines along the edges of the minute striæ or scratches which the microscope detects even on polished surfaces. This is another proof how vapour is attracted by sharp edges, for the sides of those striæ are such. Whether or not these facts had any relation to that observed by M. Daguerre, of the action of vapour at an angle of 45°, Mr. Talbot did not pretend to say, but thought them worthy of being mentioned to the Section. He observed, that it had been repeatedly stated in the *Comptes Rendus* of the French Institute, that M. Daguerre's substance was greatly superior in sensitiveness to the English photogenic paper. It now, however, appeared that this was to be understood in a peculiar sense, inasmuch as the first or direct effect of the French method was very little apparent, and was increased by a subsequent process. This circumstance rendered it difficult to institute a direct experimental comparison between them. If it could be accomplished, he doubted whether M. Daguerre's substance would be found much more sensitive than his. The present degree of sensitiveness of the photogenic paper was stated to be as follows: it will take an impression from a common argand lamp in one minute, which is visible though weak. In ten minutes the impression is a pretty strong one. In full daylight the effect is nearly instantaneous. M. Arago had stated that M. Daguerre had obtained some indications of *colour*. Mr. Talbot thereupon referred to his paper to the Royal Society, read last January, and published in the *Athenæum* (No. 588), wherein he had stated the same thing, which M. Arago has omitted to mention. Since then, more considerable effects of colour have been noticed. In copying a coloured print the colours are visible on the photograph, especially the *red*, which is very distinct. Some descriptions of photogenic paper show this more than others; but no means have yet been found of *fixing* those colours, and sunshine reduces them all to an uniformity of mere light and shade. Sir John Herschel has formed images of the solar spectrum, in which the change of colour is seen from end to end of the spectrum, but most clearly at the red end. Mr. Talbot then mentioned a kind of photogenic pictures which afford a very capricious phenomenon. The objects are represented of a reddish colour on a white ground, and the process leaving the pictures in such a state that they are neither *fixed*, nor yet the contrary, but in an intermediate state; that is to say, that when they are exposed to sunshine they neither remain unchanged (as *fixed* pictures would do), nor are they destroyed (as *unfixed* pictures would be); but this singularity occurs, that the white ground remains unaltered, while the colour of the object delineated on it changes from reddish to black with great rapidity, after which no further change occurs. These facts (he thought) serve to illustrate the fertility of the subject, and show the great extent of yet unoccupied ground in this new branch of science.

An animated conversation ensued. The President asked several questions of Mr. Talbot, tending to remove difficulties; but the questions and replies succeeded each other with such rapidity, that we could scarcely catch their import.—Prof. Forbes observed, that this communication involved subjects in which he felt deep interest, and asked Mr. Talbot whether he thought it would be possible to form extensive surfaces similar to that described, which turned yellow by heat, and whether its sensibility would exceed that of the ordinary chemical sympathetic inks.—Mr. Talbot did not know whether it would be possible to form extensive surfaces, but its sensibility to heat and cold was very great, and it had an advantage over any of the sympathetic inks with which he was acquainted.—Prof. Forbes then inquired, whether the rings and fringes described by Mr. Talbot as surrounding the particle of iodine, were those of Newton, or not rather those of Nobili,—Newton's reversed; and he also wished to know, whether he had rightly apprehended the order in which Mr. Talbot had stated them to change colour—was the olive-green developed on the inside, and the blue on the outside of the rings and fringes, or was it the reverse order?—Mr. Talbot replied, that the rings were, as Prof. Forbes said, those of Nobili, and not properly Newton's, he had used the phrase in the loose sense; the olive-green also was on the outside, and the blue on the inside. Mr. Talbot then gave some remarkable instances of the extreme sensibility of the paper he was able to produce, and said, that as the actual process of Daguerre had only been made public within the last few days, he had not been able to learn whether this sensibility could be surpassed. Some of the specimens exhibited in the Model Room had been completed in one minute, and some of the most finished in five. It was very remarkable, that time seemed to injure some, and some kinds of the paper recovered their whiteness again after having been blackened by exposure to light.—Prof. Forbes, who had, within the last fortnight, witnessed the exhibition of Daguerre's method, gave some interesting details, particularly as to the rapidity and fidelity with which views upon the Seine had been copied; and concluded by saying, that he believed that artist prided himself nearly as much on the improvements he had made in the construction of the camera obscura, as on his skill in making the photogenic drawings, although these improvements had not as yet been made public.

Section B.—CHEMISTRY AND MINERALOGY.

President—Professor Graham.
Vice-Presidents—Prof. Johnstone, Mr. Richard Phillips.
Secretaries—Golding Bird, M.D., J. B. Melson, M.B.
Committee—J. Kaye Booth, M.D., R. H. Brett, Ph. D., Prof. Christison, J. Birt Davies, M.D., Prof. Daubeny, Rev. J. P. Lee, Prof. Hermann Meyer—Prof. Vogel, Prof. Preisser—Mr. H. L. Pattinson, G. O. Rees, M.D., Prof. Schönbein, of Bâle, R. D. Thomson, M.D., Prof. Whewell, Messrs. J. Woolrich, H. H. Watson.

The President opened the Section with some observations on the recent progress of Chemistry. Chemistry, he observed, was certainly advancing with unprecedented rapidity, both in its theory and applications. The organic department was the most productive, and at the present moment engrossed almost exclusively the attention of chemists, and was likely, from the important results it afforded, to continue to do so. Without entering into any detail respecting particular discoveries, he might allude to what appeared to him the two great features of the recent progress of organic chemistry, connected as these were with the names of two illustrious Foreign Members of the Association, one of whom he expected would be present at this meeting, and the other had assisted at their meetings on a late occasion. He alluded to that happy generalization of M. Dumas—*the law of substitutions*, which, in his own hands and those of others, had been the clue to so many discoveries. He had first applied it to the action of chlorine upon organic compounds, finding that, when chlorine acted upon these bodies, for every atom of hydrogen that was abstracted in the form of hydrochloric acid, an atom of chlorine was left in its place. The same doctrine had been successfully applied to the action of oxygen, and other elements, upon the same bodies. Thus, in the oxidation of alcohol in the acetous fermentation, hydrogen was withdrawn in the form of water, by combining with oxygen; and at the same time the hydrogen was replaced by an exactly equivalent quantity of oxygen. The same law had led M. Dumas to his most recent discovery, that of chloro-acetic acid,—an acetic acid, in which chlorine was substituted for oxygen. But certainly one of the most interesting and beautiful applications of the doctrine of substitutions, was that made lately by M. Regnault, in elucidating the history of the chlorides of carbon. For the original discovery of these compounds, the science was indebted to Mr. Faraday. One of them, which contained its two elements in the ratio of single equivalents, had been named the protochloride of carbon. It appeared a body of the extreme simplicity of composition expressed in its name. But what was its real nature? M. Regnault had traced it through various compounds, all produced by the action of chlorine upon olefiant gas, by the abstraction of more and more hydrogen, and the substitution of a corresponding quantity of chlorine, till the whole four atoms of hydrogen of olefiant gas were replaced by chlorine. This view, which represented the protochloride of carbon as consisting of four atoms of carbon and four of chlorine; or olefiant gas, with its hydrogen replaced by chlorine, was consistent with the observed density of its vapour. Olefiant gas, also, it would be recollected, had the four atoms of carbon belonging to alcohol, from which it was formed; so that the protochloride of carbon had the carbon of alcohol, from which it was primarily derived, and thus preserved, after numerous mutations, the most distinct traces of its origin. The difficulty was now explained why the chlorides of carbon could not be formed by the direct union of their elements. They belong to the series of alcohol. The other characteristic feature of the recent progress of organic chemistry, to which he alluded, was the extension of the *Binary theory of the constitution of bodies*, under the powerful advocacy of M. Liebig. This was the theoretical resolution of bodies, apparently the most complex, into not more than two proximate constituents, one of which, also, was generally a simple substance. There could be no doubt, that compound radicals would be the basis of the classification of organic compounds; and that thus the same simplicity of arrangement would be introduced into organic compounds as already existed in the metallic combinations of inorganic chemistry. While adverting to recent discoveries, it was impossible to avoid noticing that of the new metal *Lantane*, the 55th elementary body, of which, through the kindness of Dr. Hermann Meyer, of Berlin, he was enabled to present some of the preparations to the Section. In the history of the useful applications of chemical science to the arts, the last year would be memorable for various improvements and inventions connected with the soda process. Sulphuric acid, which was the key to so many important chemical products, was chiefly prepared from the sulphur of Sicily, the supply of which had been suddenly much reduced by some fiscal regulations of the Sicilian government. This had excited the ingenuity of our chemical manufacturers, and had led to the invention of several new processes for soda, which, whatever they may prove as manufacturing processes, were certainly possessed of considerable merit as chemical discoveries. One of these new processes of peculiar interest, was that of Mr. Gossage, of this neighbourhood, for the recovery of the sulphur from soda-waste, which promised not only a great saving of material, but a benefit of another kind, in abating, or entirely removing, the nuisance from the escape of muriatic acid into the atmosphere in the ordinary soda-process.

A communication was then read from Prof. Hare, of Philadelphia, 'On the Preparation of Barium, Strontium, and Calcium.' By means of the alternate action of two deflagrators, each of 100 pairs, containing more than 100 square inches of zinc surface, assisted by refrigeration, Dr. Hare has procured amalgams of these metals from their chlorides, and by distillation in an iron crucible, included in an air-tight alembic of the same metal, has extricated them from their mercurial solvent. He observes that they are so oxidizable, that, in order to see their brilliant white metallic colour, the eye must follow close upon the track of the file or the burnisher; almost as soon as a fresh surface is exposed, it assumes a straw colour, like that given in the first stage of oxidizement, and is soon completely obscured by the generated oxide. Of several kinds of naphtha in Dr. Hare's possession, only one, which he had distilled from a residue of the distillation of potassium, does not act upon these metals. After being some time in naphtha, their effervescence with water is much less active. Under such circumstances, they re-act at first more vivaciously with hydric ether than with water or chlorohydric acid, because the ether removes a resinous coating derived from the naphtha. The Professor goes on to state the results of his experiments, but as none of his results are of a novel character, and as we believe they have been already published in *Silliman's Journal*, it is unnecessary to particularize them.

A letter was read from Mr. Mallet, of Dublin, who, in conjunction with Prof. Davy, was appointed two years ago to make experiments on the action of water upon cast iron, and who furnished a very able report last year. Mr. Mallet stated in his letter that he was proceeding with his experiments, and requested a continuance of the grant of 50*l.* allotted for these researches.

A letter was read from Dr. Reid, of Edinburgh, accompanied by a Chemical Abacus, which he had found useful in introducing his pupils to a precise knowledge of the constitution of the more important chemical compounds. It consisted of a frame of wood, across which wires were placed, and upon which beads were strung, precisely similar to the instrument employed by Chinese clerks, and which is to be found in most museums. Each wire

corresponds to a chemical element, and the beads to atoms, while the names of the elements are placed on the frame at the extremities of the wires.—Dr. Daubeny suggested that the instrument might be improved, by having the beads of different colours, to correspond to the different elements.

Mr. Coathupe, of Bristol, read a communication 'On an Improved Method of Graduating Glass Tubes for Eudiometrical Purposes.' The apparatus for this purpose consists of a truly bored cylindrical tube of iron, into which an iron piston is accurately fitted. Upon the rod of this piston a screw has been cut, with a good pair of dyes, throughout its entire length. The rod was then filed into a triangular form, leaving a sufficiency of the threads of the screw at the rounded angles for an iron nut to traverse with security and freedom. To the upper extremity of this iron cylinder a cap of the same metal is screwed, and into this cap is screwed an iron stop-cock. To the stop-cock is attached a glass graduated measure, with a narrow lip, by means of an iron connecting socket. Near the opposite extremity of the cylinder, an iron diaphragm, of about a quarter of an inch in thickness, is inserted, and is fastened in its place by a side screw or pin, and through this diaphragm a triangular shaped hole is made, through which the piston rod can slide easily up and down, but without lateral shake. Below the diaphragm, and at the extremity of the cylinder, the nut is inserted, whose action propels or retracts the piston without the possibility of the piston itself deviating from a right line. This nut enters the cylinder to the depth of about half an inch, and around the entering part a deep groove, in the form of the letter V, is turned, into which the pointed ends of three steel screws enter through the exterior of the cylinder at equal distances, in such a manner that the nut can be revolved freely, but cannot be otherwise displaced. From the entering part of the nut a projecting portion forms a shoulder, which is graduated into equal parts: this projecting portion may be of any diameter greater than that of the cylinder. On the exterior of the cylinder an index is fixed, by means of which any number of revolutions of the nut, or any number of equal parts of a revolution, can be ascertained. To prepare this instrument for use, the piston is to be retracted to its lowest position, and the cylinder is to be filled with mercury, (without air bubbles,) by pouring a sufficient quantity of this metal into the graduated glass that is attached to the stop-cock, and turning the plug for its admission within the cylinder. If, when the cylinder is full, and while some mercury still remains within the graduated glass measure, we turn back the plug of the cock, we get the air-way of the plug filled with mercury; and by pouring off the superfluity, we have the instrument in a proper state to commence graduating any tube for laboratory purposes. Thus, if the tube to be graduated be about one-third of an inch in diameter, if we open the communication between the cylinder and the measure, and propel the piston by one whole turn of the nut, and then close the communication between the cylinder and the measure by turning the plug of the cock, we have within the measure a quantity of mercury, which, when poured into the tube to be graduated, will give a tolerably long space for the first division; and such similar spaces may be repeatedly marked, by repeating the process, until the whole tube be equally divided from end to end. The author referred to Dr. Hare's method of procuring minute quantities of mercury of equal bulk, by propelling a graduated rod of cylindrical wire through a stuffing-box into a glass tube, which had been drawn to an almost capillary orifice at one end, and then filled with mercury, and collecting the displaced globules as the rod was advanced; but considered his own method preferable.

Prof. Graham considered that this method of graduating glass tubes might be advantageous in laboratories.

Mr. Coathupe also exhibited an apparatus for determining the quantity of carbonic acid in the atmosphere.

It was objected to this apparatus, that it was not susceptible of complete accuracy, and that therefore it would not answer for the purpose intended, which required great precision.—Mr. Pearson recommended, for the purpose of detecting accurately the quantity of carbonic acid in the atmosphere, a method which he had adopted with success. He prepared a tube, filled with lime water, and hermetically sealed. He introduced this into the vessel containing the air to be examined, and then broke the tube by a smart blow.—Mr. Benson stated, that water saturated with sulphate of magnesia possessed very little absorbing power over carbonic acid, and recommended this instead of mercury for testing in manufactories.—Prof. Graham considered the first communication of Mr. Coathupe the more valuable of the two, and recommended Dr. Dalton's method of determining the carbonic acid of the atmosphere.

Dr. Mackay, of Birmingham, exhibited a specimen of a bark—he did not state from what tree derived—and of two oils which he had distilled from it.

The Baron Eugene du Mesnil gave a description of a Safety-Lamp, invented by him in 1834. He stated, that he had presented it to the French government in 1837, and that it had been now adopted, after a favourable report upon it by M. Ch. Combes.

This lamp consists of a body of flint glass, defended by a dozen of iron bars. The air is admitted by two conical tubes, inserted at the bottom, which are capped with wire gauze, and enter by the side of the flame. The latter rises into a chimney, which has a piece of metal placed in the form of an arch over its top; the chimney, however, being quite open. The consequence of this construction is, that a strong current is constantly passing up the chimney. When carburetted hydrogen passes in, the fact is discovered by numerous small explosions, and the whole glass work is thrown into vibrations which emit a loud and shrill sound, which may be heard at a very considerable distance.

Prof. Graham stated, that the novelty in Baron du Mesnil's lamp was, the circumstance of the chimney being quite open. He considered, that the lamp of Davy was left almost perfect by that philosopher, and that all accidents proceeded from carelessness. He alluded to the deleterious effects of the *after-damp*, or carbonic acid left in the atmosphere of a mine after an explosion, which is believed to occasion often greater loss of life among the miners than the original explosion, and often prevented assistance being rendered in case of accidents. In many cases, it was certain that the oxygen of the air was not exhausted by the explosion, although, from the presence of 5 or 10 per cent. of carbonic acid, it was rendered irrespirable. The atmosphere might, therefore, be rendered respirable by withdrawing this carbonic acid, and he suggested a method by which this might be effected. He had found, that a mixture of dry slaked lime and pounded Glauber's salts, in equal proportions, has a singular avidity for carbonic acid, and that air might be purified completely from that deleterious gas, by inhaling it through a cushion of not more than an inch in thickness, filled with that mixture, which could be done without difficulty. He suggested the use of an article of this kind by persons who descended into a mine to afford assistance to the sufferers, after an explosion: indeed, wherever the safety-lamp was necessary, and the occurrence of an explosion possible, the possession of this lime-filter would be an additional source of security.

Section C.—GEOLOGY AND PHYSICAL GEOGRAPHY.

President—Rev. Dr. Buckland.
Vice-Presidents: (for Geography)—Mr. G. B. Greenough, *(for Geology)* Messrs. H. T. De la Beche, Leonard Horner, Charles Lyell.
Secretaries—G. Lloyd, M.D., Messrs. H. E. Strickland, C. Darwin.
Committee—Messrs. J. McAdam, J. E. Bowman, F. Downing, J. R. Corrie, M.D., Messrs. W. Hopkins, R. Hutton, M.P., W. Matthews, R. I. Murchison, *Gen. Sec.*, The Marquis of Northampton, Messrs. J. Marshall, J. Phillips, W. Sanders, Dr. W. Smith, Messrs. G. W. Wood, M.P., J. Wickenden, J. Yates.

The President submitted to the Section a recommendation from a society in Bradford, that the attention of members of Museums in provincial towns should be directed chiefly, if not solely, to the collection of specimens from their own immediate vicinities. Many advantages would result from the adoption of this. Each district of the country would become more fully known; its productions would be at once placed in such a form as to give the necessary knowledge to those seeking for information, and the co-operation of the inhabitants of each district would be secured in the undertaking. It was particularly recommended to Birmingham, from its important location in the coal and iron districts; and an admirable example was afforded in what had been done at Newcastle-on-Tyne.

Mr. Greenough advocated the importance of topographical museums. He mentioned that at the Bureau des Mines at Paris, the specimens from each department in France are kept distinct. No similar collection is to be found in Great Britain, not even in the British Museum. It is also of importance that not only mineralogical specimens be arranged topographically, but that there should be a similar disposition of organic remains, as different fossils may occur in the same stratum in different districts.—The Marquis of Northampton stated the difficulty in having such a collection placed at present in the British Museum, from want of room. He was fully aware of the importance of local collections, and wished that all provincial societies would adopt the recommendation; and that in a few years the British Museum might be able to have a national one, similar to the French.

Dr. Buckland laid before the Section the last number of M. Agassiz's work on Fossil Fishes, and spoke of the merits of that gentleman, who had sacrificed very flattering prospects in mercantile life to a love of science, being content to live almost in poverty, devoting his slender means to the furtherance of his undertaking. M. Agassiz had received pecuniary assistance from the Association; and to that body, as well as to the English subscribers to his work, he was most grateful, for, without such assistance, he must have abandoned the undertaking, so valuable to the scientific world, and especially to Geology. Dr. Buckland stated the importance of fossil fishes to the geologist, their scales being preserved when their skeletons were destroyed; and made some observations on the adaptation of the covering of animals to the medium in which they live. He adduced the minute scales of the eel, covered over with mucus, to protect it in the mud,—this mucus preventing the scales from being grated or injured.

Mr. Lyell then read a paper 'On the Tubular Cavities filled with Gravel and Sand in the Chalk near Norwich.'

The chalk near Norwich is covered with gravel, sand, and loam, of variable thickness, much stained with iron, occasional masses of ferruginous sandstone being interstratified, in which are casts of the shells of the Norwich crag. The shelly crag itself forms here and there part of the same deposit. The outline of the chalk, at its junction with the incumbent gravel, is very irregular. In some places, tubular hollows, having the form of inverted cones, and filled with gravel and sand, are prolonged downwards to various depths into the chalk. These cavities vary in width, from a few inches to eight yards and upwards, and in depth from a few feet to more than sixty. Some are tortuous, but most of those at Eaton, two miles west of Norwich, are perpendicular. The materials filling the pipes agree precisely with those covering the chalk, with the exception that in the pipes they are unstratified. The pebbles in the gravel consist of rounded flint and quartz; but no shells or pieces of chalk, or any calcareous substance, occur in the pipes. In general, coarse sand and pebbles occupy the central parts of each pipe, while the bottom and sides are lined with a fine ferruginous clay, which, however, is permeable by water. This clay contains no calcareous matter. The chalk, for the distance of several inches, or even sometimes four or five feet, from its junction with the sand pipe, is in a moist and softened state, and contains a slight mixture of fine sand and clay, by which it is somewhat discoloured. The chalk, at points more remote from the tubes, is white, pure, and perfectly soluble in acids. The pipes, which do not exceed a foot and a half in diameter, are often crossed by horizontal layers of flint nodules, which have remained in situ, while their chalky matrix has been removed. From this circumstance, the author infers that the pipes were due to the corroding action of water containing acid, which could not dissolve flint. But it is clear that the tubes were not first excavated to their present width and depth, and then filled subsequently and at once with gravel, for in that case the siliceous nodules would have been found in a heap at the bottom of each large cavity, having been derived from all the intersected layers of flint. This never happens, the larger flints being invariably dispersed irregularly through the gravel and sand which fills

the tubes. Mr. Lyell, therefore, inferred, that the excavation and filling of the pipe proceeded contemporaneously and gradually, and that the flint nodules, when removed from their chalky matrix, subsided so as to rest upon sand and gravel which had previously sunk. As proving that the contents of the sand pipes came into their present position by slowly subsiding, the author mentioned the fact of strata of gravel elsewhere horizontally bending downwards into the mouth of a pipe, so as to become for a short space quite vertical within the pipe. Mr. Lyell is of opinion, that the tubes, at least of some of the larger and deeper ones, were caused by springs impregnated with carbonic acid, which rose upwards through the chalk. But afterwards, when these springs ceased, the descent of rain-water, percolating the gravel, carried fine particles of sand and clay downwards, and deposited them at the bottom and sides of the tube, at all those points where the water was absorbed by the surrounding chalk. Some of the finer particles being carried into the chalk itself, caused the impurity and discoloration of that rock near the pipes.

Mr. De la Beche mentioned that similar appearances are observed in other formations, as in green sand, near Charmouth. But water charged with carbonic acid must have different effects on different rocks; so that the same explanation could not be applied to fissures in siliceous, as to those in calcareous strata. Water from above, and which is always impregnated with carbonic acid, may have taken up the chalk, and then, from the porosity of that rock, have oozed downwards, carrying with it the displaced material, with the exception of the small quantity of clay and silex contained in all chalk, and which remained behind as the lining of the cavities.—Mr. Phillips assented to the supposition that these fissures were not the result of mechanical violence; similar openings occur in the oolites, with layers of clay. There might have been originally a commencement of the fissure by the action of large stones on one another, as we see in waterfalls and on the sea shore.—Dr. Buckland alluded to the large sand pits in the hills near Beaconsfield, as instances of a similar kind. At Shotover, near Oxford, the oolite has pits of a like kind, under Kimmeridge clay, with a thin stratum, still clayey, of a different nature. Near Caen, in Normandy, appearances such as those described by Mr. Lyell occur, only that, in place of flints, there are nodules of chert. At Shotover there seems to be a beach on the surface of the oolite, bearing the marks of watery action, grinding the stones together.—Mr. Strickland mentioned that, in the Greek islands, where there are no argillaceous rocks, the fissures in the limestones are found to contain clayey matter.—Mr. Yates alluded to the *organ pipes* at Maestricht, which, however, have not been fully examined.

Mr. J. E. Marshall exhibited a section across the Silurian Rocks in Westmorland, from Shap Granite to Casterton Fell. He remarked, that the rocks to the south of Shap Fell, and the heads of Windermere and Coniston lakes, have been classed by Prof. Sedg wick as belonging to the Silurian system; and the boundary between them and the Cambrian rocks is described as accurately defined by a remarkable band of fossiliferous limestone, running from the Shap Granite to the flank of Black Comb. Mr. Marshall was induced to examine these rocks more minutely, by the recommendation of Mr. Murchison, who had expressed his opinion, that the rocks near Kirkby Lonsdale are decidedly upper Silurian. A number of fossils were collected from the Coniston limestone, proving it on examination to be of the age of the Caradoc sandstone; and others from Benson Knott, near Kendal, were identified with those of the Upper Ludlow rock. The following may be taken as a sketch of the rocks overlying the Coniston limestone.

Llandeilo Flags, or Caradoc Sandstone.	1. Coniston Lime.	1. Dark blue limestone in thin beds interstratified with slates, and having many fossils..100 to 300 feet.	
		2. Soft black slate with numerous joints ..300 to 700 feet.	
		3. Compact blue flag-stone, with iron pyrites in the seams..600 to 800 feet.	
	2. Bluish Slates.	Hard gray siliceous slates, with uneven cleavage, and frequently with stripes of a lighter colour, containing veins of quartz	4000 to 5000 ft.
		Containing at foot of Coniston Lake a weak band of calcareous slate, with fossils	

With respect to the subordinate beds at Bowness and at Potter Fell, they are suddenly thrown up on edge, and dislocated; between Ambleside and Kendal they may be observed nearly in a vertical position. A narrow stripe of carboniferous limestone overlies conformably the old red sandstone breccia; but at its termination is cut off and thrown on edge by a fault ranging in an E.N.E. direction. The cliffs of breccia at Casterton, in the course of the Lune, where the river runs along the chasm formed by a great fault, dip at moderate angles to the S.E. Casterton Fell consists of an anticlinal ridge of slate running N.N.E., with a S.E. dip on its eastern, and a N.W. one on its western side, at various angles. The carboniferous strata do not therefore conform with the Casterton slates. By the great fault in the valley of the Lune, the strata are depressed on the eastern side, and shifted northwards in a lateral direction. Near Kendal the tilestone may be seen cropping out in an abrupt escarpment; and the Benson Knott, or Upper Ludlow rock, rising conformably from beneath it. This last rock is a hard arenaceous slate, with many fossils, and much jointed. A great fault ranges down the valley of the Kent, bringing the carboniferous limestone into contact with the lower part of the Benson Knott rock. A great transverse fault ranges E.N.E. down the course of the Mint, cutting off this rock suddenly; and at about a mile distance from this river, the breccia and limestone are again cut off by another fault, parallel to the first. The remarkable contortions seen near the top of the principal ascent of Shap Fell, appear to be in the upper beds of the Coniston Flag, a portion of which is metamorphosed into syenite and felspar porphyry. Mr. Marshall noticed, at the end of the memoir, a singular contrast in the direction of the cleavage of the upper Silurian rocks, compared with that of the lower Silurian or Cambrian. In the two last it is most uniformly about N.N.E., but in the upper Silurian and tilestone W.N.W.

Prof. Phillips stated, that he had first called the attention of geologists to the occurrence of fossils at Kirkby Lonsdale. He advocated the comparison of Silurian rocks in different districts, as variations should be found in the proportion of the different members of that system. It is possible that no Wenlock limestone occurs in Westmorland, although the fossils from Coniston Head may, on careful examination, be found to resemble those of that rock. He considers there is great difficulty in tracing a fault in the valley of the Lune.—Mr. Murchison considered that it is doubtful if the upper Ludlow occupies all the space marked in the section. In this part of England he thinks it unlikely that there is any disruption between the Silurian system and the tilestone, although Prof. Sedgwick is of an opposite opinion.—Prof. Phillips conceived, that in the case of the fault in the valley of the Lune, a disturbance may have taken place in the ancient bed of the sea, which caused an alteration in the arrangement of the strata, but that there is no real fault.—Mr. Murchison observed, that Casterton Fell must be upper Silurian, from its containing a peculiar species of orthoceratite. It has frequently a hardened, altered appearance in its rocks, from their being penetrated by trap.

Mr. Strickland read the following queries, on which he solicited information. They relate to the superficial gravel in the neighbourhood of Birmingham.

1. Does the gravel near Birmingham ever contain chalk flints, fragments of oolite, &c. which may indicate a southern origin, or is it wholly of northern extraction?

2. Does it ever contain marine shells?

3. Are these shells of existing or extinct species?

4. Does it ever contain bones of terrestrial mammalia, or fresh-water shells?

5. What are the circumstances of position, material, &c. of the gravel in which mammalian bones or fresh-water shells are found? and is it distinguishable in any respect from the gravel in which marine remains are found?

6. Are mammalian remains ever found in company with marine shells?

The object of these inquiries will be best understood by referring to Vol. VI. of the Association's Reports.

Section D.—Zoology and Botany.

President—Professor Owen.
Vice-Presidents—Mr. J. E. Gray, Dr. Graham, G. B. Daubeny, M.D.
Secretaries—Messrs. E. Forbes, R. Patterson, W. Ick.
Committee—Mr. C. Babington, Rev. W. T. Bree, Messrs. G. Barker, B. Blood, F. Bossey, M.D., Messrs. W. Bowman, J. E. Bowman, D. W. Crompton, J. R. Corrie, M.D., Messrs. J. Goodsir, J. Hodgson, G. Webb Hall, G. B. Knowles, E. Lankester, B. Maund, L. Parker, F. Ryland, Prof. Rymer Jones, Messrs. R. Taylor, F. Russell, H. E. Strickland, N. A. Vigors, M.P.

A paper was read, 'On the Formation of Woody Tissue,' by Mr. Lankester.

The tissues of plants, for the sake of convenience, are divided into five, but the origin of these may be all traced to the simple cell. How they are formed from the simple cell is an undecided question, especially with regard to woody tissue. Du Petit Thouars supposes, that woody fibre is formed by the buds and leaves, and sent down by them between the bark and wood of the tree, whilst other writers suppose that it is formed from the bark or wood. The most prominent features of woody tissue are its length, and the hard nature of the secretions deposited in its interior. These points do not constitute an essential difference between this and cellular tissue, as we find the latter lengthened in the form now called Pitted tissue, and hardened in the endocarps of Amygdalæ, &c. If by the term woody tissue, all hardened lengthened tissue be included, then we find it present in many instances, where neither buds, leaves, nor bark can be said to exist, as in Cryptogamia, especially Boleti. Again, it is found in many parts of Phanerogamous plants, as the pericarps of the cocoa, beech, and other plants, which in those parts are destitute of leaves. The author had also found woody tissue in abundance in the leafless Monotropa, and in many species of Cactaceæ. Other objections may be urged against the theory of Du Petit Thouars, as the fact of wounds in trees being filled up at their lower as well as their upper lip. In trees that had been felled, the author had observed the production of fibrous tissue, independent of leaves or buds: specimens of this were exhibited to the Section. The author then detailed the appearances he had observed in several trees that he had ringed in the spring, when the sap was rising, and had cut down in August. In these trees he found that woody tissue was connected with a cellular formation, in both the lower and upper edges of the wound. The woody tissue being evidently formed subsequent to the ringing. The last occurrence to which the author directed attention was, the existence of knobs of wood in the bark of beech and other trees. These excrescences are of all sizes, and when first formed are cellular; they gradually harden, and at last present layers of contorted woody fibre. They have a regular bark of their own filled with sap during the spring, and present concentric circles of woody tissue, representing their yearly growth. Many of them put forth buds, and some few leaves, but by far the greater number have neither buds nor leaves. Sometimes they congregate together in a mass, each nucleus having a separate series of concentric layers surrounding it. Although from rapid growth the compound knobs are found in contact with the wood of the tree, the single knobs are seldom found in this position. These knobs have been called by Dutrochet, embryo-buds, and used by him as an argument against Du Petit Thouars' theory, and Dr. Lindley admits their existence to be one of the greatest objections of which he is aware. The conclusions to which the author came, from taking a view of all the facts which we are in possession of at present, on the formation of woody tissue were:—1. That the requisites for the formation of wood are a living tissue developing elongated fibres, a tissue forming and depositing secreted matter, and exposure to the influence of external stimuli. 2. That the secreted matters are more easily brought under the influence of external stimuli in the younger tissues; hence the importance of leaves. 3. That neither bark nor leaves are essential to the formation of woody tissue.

The paper was accompanied by several preparations and specimens, which were exhibited to the Section.

Mr. Babington was glad to find, that the knobs in trees had excited attention. He believed that they afforded evidence of the unsoundness of Du Petit Thouars' theory. He had never been inclined to adopt the view of wood being a descending fibrous formation of the leaves. If this were the case, the

stock of a grafted tree should have wood formed round it like the graft; but this was not the case, as the wood of the stock remained the same.—Dr. Bossey thought the views of Du Petit Thouars were beset with objections, besides those which Mr. Lankester had brought forward. The oriental plane shed its bark from increase in the wood of the trunk: if this wood was formed by the leaves, the shedding of the bark would take place from the upper part of the stem first, and not from the middle and lower portions, as it was now found to do.

'Notice of Zoological Researches in Orkney and Shetland during the month of June, 1839,' by Edward Forbes and John Goodsir.

Mr. Forbes commenced by stating that, in the month of June last, he and Mr. Goodsir visited the islands of Orkney and Shetland, with a view to the investigation of the marine zoology of the northernmost district of Britain. Though their stay was limited to three weeks, and though the season was unfavourable to the examination of such animals as are brought up by the deep-sea (*Haaf*) fishing lines, their success proved greatly beyond their anticipations; and they brought forward their short notice before the Section, as much with the view of inducing others to investigate a district at once accessible and abounding in objects of discovery, as for the purpose of making known the additions to the British Fauna resulting from their excursion. Having directed their attention almost exclusively to the invertebrate animals, Mr. Forbes stated that, in regard to the mollusca, they had found five species of the genus Eolida. Of these, four are undescribed, the other being *Eolida papillosa* of authors, which abounded in Shetland, under stones at half tide, whither it appeared to resort for the purpose of spawning. Along with it occurred a nearly allied, but very distinct species, which they proposed to name *Eolida Zetlandica*. In shape, it nearly resembles the *Eolida papillosa*, but is smaller. The latter species has twenty or more rows of branchiæ on each side, whilst the new species has only twelve or thirteen rows of lateral branchiæ, six or seven in each row. The colour of the *Eolida Zetlandica* is also much darker. The thread of spawn is arranged in a similar manner, but is much narrower, and easily distinguishable. The other three new species belong to a different section of the genus. One is nearly allied to the *Eolida pedata* of British authors. Mr. Forbes stated that they had named it *Eolida coronata*. Body linear, white branchiæ arranged in five or six lateral fasciculæ, purple tipped with white, and lineated with blue. Superior tentacula yellow, each with eight broad membranaceous wings. Inferior linear, white; length, one inch. *Eolida foliata*—a minute species, having six rows of leaf-shaped branchiæ, two in each row. Tentacula, simple linear. *Eolida minima*—a minute species, having seven long contractile branchiæ on each side. Its oral tentacula in the form of broad lobes. Of these three Eolidæ, the authors stated that the first is found under stones at low water, and that the other two were obtained by dredging in seven fathoms water;—all from Shetland. They found no Eolidæ in Orkney. Of the genus Euplocamus, they found one species, allied to the *Euplocamus pulcher* (*Triopa clavigera* of Johnston), but differing from that species, in having its branchiæ, both lateral and dorsal, tipped with yellow. Its back is white, spotted with yellow. Of the genus Doris, they found two species, one the *Doris pilosa* of Müller, and the other *Doris bervicensis* of Johnston. The latter belongs to the same division of the genus with *Doris nodosa* of Montague, and should constitute with it a separate genus, or at least subgenus, which might be called Goniodoris. They found only one new testaceous mollusk, a species of Velutina; it has an ovate oblong shell, covered with a fine woolly epidermis, not ridged longitudinally. Spine very small, depressed. Aperture large, ovate, margin of columella, and upper part of outer lip, thickened. Shell, horn-coloured. Animal, orange. The authors have named it *Velutina elongata*. Ascidiæ abound in the north; the more common species is the *Ascidia intestinalis*. Along with it they found three species, which, there is reason to believe, are undescribed. The first is a very beautiful species, globular, white, covered with spinous processes, ending in a star of five cartilaginous filaments; the orifices are bright rose colour. It is about an inch in diameter, and is not uncommon in rather deep water. The authors propose to name it *Ascidia echinata*. The other two are equally distinct. The first—*Ascidia rugosa*—is red, opaque, elongated, its surface warty. The second—*Ascidia rubens*—is red, translucent, elongated, with distant apertures, and grows to the length of eight inches. They are both from Orkney. Of Annelides, the authors found great numbers; such as they collected they intend submitting to Dr. Johnston, as the best British authority on that class. They observed the Planariæ in great numbers. Among others, was the beautiful *Planaria atomata* of Müller, not before recorded as British. Among the Radiate animals they were especially successful. The genus Holothuria holds its British court in Shetland, and the king of them is an enormous species, which the authors name *Holothuria grandis*. This splendid animal is fully two feet long, when extended; it is of a deep purple colour; it has ten triangular frondose tentacula, purple spotted with white; its body, between the rows of suckers, is almost smooth. The other new species of this genus observed were—2. *Holothuria fucicola*—body ovate, purple, smooth; tentacula ten, deep brown, ovate; suckers numerous in each row; size, three inches; on the stems of Laminariæ, in seven fathoms water—Shetland. 3. *Holothuria brevis*—body ovate, arcuated, very short, pinkish white; suckers few; tentacula long, pinnate at their extremity, varying in number; length, half an inch. 4. *Holothuria fusiformis*—body linear, white, rough; suckers numerous; tentacula ten, short, triangular. 5. *Holothuria lactea*—body cylindrical, angular; milk white; suckers few; tentacula ten, short, ovate. 6. *Holothuria pellucida* of Müller. Along with Holothuriæ, the authors dredged the *Priapulus caudatus*, and *Sipunculus strombi*. They found no Holothuriæ or Priapuli in Orkney. Of the sea-urchins, they found only the *Echinus esculentus*, and a form which appeared to be the *Echinus neglectus* of Lamarck. The star-fishes observed were *Asterias aranciaca*, and an allied form, probably the *Asterias bispinosa* of Otto, *Stelloniæ rubens* and *violacea*, *Luidia fragillissima*, *Solaster popposa*, *Ophiura albida* and *texturata*, *Ophiocoma bellis*, *granulata*, *roscola*, *neglecta*, and a new species. The Medusæ doubtless abound in these islands in August, their proper season, but when the authors of the paper were there, they observed only *Cyaneæ capillata*, *Medusa aurita*, a new Dianæa, a new Oceanea, a new Ciliograde, of the genus Alcynöe of Rang, and a minute animal, the type of a new genus among the Acalephæ. Sponges of the genus Grantia and Holochondria abound in Shetland. From deep water, the authors obtained several specimens of *Tethya cranium*, and kept them alive in salt water, but could observe none of the contractions stated to have been seen in that species by some of the French naturalists. The most beautiful contribution to the British Fauna from the Orkneys, is a zoophyte of the family Tubulariadæ, new both as a species and genus, and the largest known form of its tribe. This beautiful animal is about four inches long, and its stem half an inch in diameter. This stem is rounded, solid, flexible, moving at the will of the animal, and somewhat contractile. It is translucent, of a pinkish white colour, lineated with brown longitudinal lines, arranged in pairs. When young, the stem is shorter, and is inclosed in a delicate, brown, corneous tube, which becomes deciduous as the animal grows larger. The lower part of the stem is broader than the upper, and roots in sand by means of a fusiform termination, sending out corneous filamentous roots. At the upper extremity, the stem becomes suddenly contracted, and the lines terminate; it then expands into an ovate head, terminating in a long pyramidal, pink trunk, at the end of which is the mouth. Round the thickest part of the head, is placed a row of about forty long, white, uncontractile tentacula, which wave about in all directions, and are not ciliated. Immediately above the circle of tentacula, is a circle of about twenty-five ramified orange processes, probably ovarian, having no voluntary motion. Above this, the trunk is covered with numerous white tentacula, very much shorter than the outer circle. Within this head, is a simple digestive cavity, not extending down so far as the large tentacula. Every other part of the animal is solid, and no part is ciliated. Beautiful and delicate as these animals appear, they are very tenacious of life. We dredged them in considerable numbers, on a sandy bottom, in about ten fathoms water, at Stromness, Orkney. The position of this animal is between Tubularia and Coryne, on the relations of which genera its discovery throws much light, as well as on the polypes in general. We propose to consecrate the genus to that great British zoophytist, Ellis, calling it Ellisia, and giving the species the appropriate name of *Flos maris*, as it may well be regarded, from its extreme grace and beauty, as the flower of the British seas. The relations of Ellisia to Tubularia, may be exhibited by the following diagram:—

Coryne—Tentacula scattered, of one kind; no tube.
Hermione—Tentacula scattered, of one kind; tube.
Eudendrium—Tentacula of one sort, regular; branched tube.
Tubularia—Tentacula of two sorts, regular; simple tube.
Ellisia—Tentacula of two sorts, regular; deciduous tube.

Mr. Gray observed, that the name Ellisia had been already applied by Brown to a genus of plants.—Prof. Jones inquired if Mr. Forbes was certain with regard to the deciduous nature of the tubes of Ellisia. It was a circumstance of an extraordinary kind, and required close investigation.

Mr. W. R. Wilde exhibited three drawings of a Peruvian Mummy, showing its different states of developement; and mentioned, that he read some time since a series of papers on the subject before the Royal Irish Academy.

Mr. Lankester then made some observations on the preparation of fishes for museums. He exhibited several specimens, which, after having taken away one side, he had allowed to dry, and assume their natural state, and then placed them on paper. The process consisted in drying the fish, then taking away their soft parts, then drying the skins, keeping them in shape by pieces of stick and cork, and, finally, varnishing them with mastic varnish, by which they become stiffened, and their colours preserved.

Mr. Gray stated, Mr. Lankester's process was not new. His uncle adopted it many years ago at the British Museum, by modelling wax and resin to the shape of the fish. A carpenter's boy had also prepared a great number, by skinning the fish, and filling it with bran, and when dry it retained its original shape. He hoped soon to have a very fine collection open for inspection at the British Museum. He thought the fish were better sewed on to the paper than glued. The preserving the colour of fish and reptiles was a great object; he had attained this by soaking them in brine, and drying them before they were put in spirit. He condemned the use of the oxymuriate of mercury in the preparation of animal substance.—Dr. Macartney recommended pyroligneous acid, instead of corrosive sublimate. In reply to a question from Mr. Forbes, Dr. Macartney observed, that Medusæ might be preserved by putting them into a solution of alum and nitre in spirit; they lost little of their size thus, and could be kept in no other way.—Mr. Forbes had seen several specimens of reptiles in Dr. Knapp's possession, which retained their original colouring. They had been preserved in spirit two months, and then taken out and dried, and placed upon cork.—Dr. Macartney observed, that solutions of alum preserved the colour of animal substances best; and Mr. Gray stated, that reptiles might be preserved by pressing, as plants, or by skinning, and filling with sand.

Section E.—Medical Science.

President—Dr. Yelloly.
Vice-Presidents—Dr. Johnstone, Dr. Rocket, Dr. Macartney.
Secretaries—Dr. G. O. Rees, Mr. F. Ryland.
Committee—Drs. Blakiston, Booth, G. Bird, Mr. W. S. Cox, Drs. Evans, Foville, B. Fletcher, Hastings, T. Hodgkin, Mr. J. Hodgson, Drs. J. Johnstone, Pritchard, Mr. J. Russell, Drs. R. S. Sargent, Vose, Mr. R. Wood, Dr. Wale.

After an introductory address from the President, one of the Secretaries read a paper, communicated by Sir David Dickson, containing, 'Abstracts of a

remarkable case of Rupture of the Duodenum, and of some other interesting cases.'

R. H. was admitted into hospital on March 3rd, at three o'clock P.M., and died before midnight. The symptoms were, severe pain in the region of the cæcum and ascending colon, hurried, restless manner, pale, haggard, and anxious countenance, short, hurried respiration, pulse weak, quick, and irregular. Depletion, followed by aperients, had been resorted to, without relief; leeching, fomentation, and warm purgatives, were resorted to without any relief, and, at half-past eleven, he sunk. It was ascertained, that he had been fighting and wrestling three days before, when he was thrown with violence on the breach of a gun. Post mortem examinations discovered the following lesions:—The stomach, bowels, and abdominal cavity were filled with gas, the descending colon was much contracted; a quantity of ingesta had escaped from four perforations in the duodenum, the mucous and muscular coats of this gut were pellucid and attenuated, as having undergone ramollissement and absorption, in consequence of which, the peritoneal coat seemed to have given way, from distension or mechanical violence; the other viscera seemed generally healthy. It is known, that sudden death frequently follows violent feats of tumbling and horsemanship, and, if examination were made, probably similar lesions would be found, or might exist without being detected, from the examination not being sufficiently minute; and thus this cause of death may more frequently occur than is generally supposed.

The next case detailed was one of Ileus, with enormous distension of the cæcum, which occupied the situation of the transverse colon. The usual symptoms of Ileus were present—viz. obstinate constipation, stercoraceous vomiting, and singultus. The ileo-cæcal valve was much diseased, being thickened, and as hard as cartilage. The cæcum, which was forced upwards, had a black and phaselated appearance, and was enormously distended. A strong membranous band, the product of previous inflammation, extended across the ileum, and firmly connected it with the meso-colon. Another case was one of *intermittent coma* from diseased brain. This case was remarkable, for the alternations of coma and excitement. The *post mortem* examination showed the arachnoid membrane to be opaque, and to be raised up from the brain by a gelatinous deposit. A considerable bloody effusion was found at the base of the brain, proceeding from the rupture of a true aneurism of the anterior artery of the cerebellum, near its junction with the basilar artery. The coats of the artery exhibited distinct ossific deposit—the cerebellum, on the left side, was wasted and softened in structure, and of the appearance of curdy pus. The aorta was found extensively invaded with ossific degeneration; scales of bone as large as a sixpence being separable from it, and its elasticity much impaired. Other cases of coma were detailed, in which depositions of a cartilaginous and tubercular nature were found in different parts of the brain. In a case of Phthisis, the *foramen ovale* was found open; cyanosis did not exist; and the patient, a pensioner, had completed his due period of servitude, and had risen to the rank of sergeant, without suffering any inconvenience from the free communication between the right and left sides of the heart.

The next case was one of Phlegmonous Erysipelas, occupying the arm and thoracic muscles of left side, and remarkable for its extreme rapidity. Two cases of severe abdominal disease were also detailed. In the last was observed an extensive deposit of semi-cartilaginous firmness, occupying the subserous cellular tissue; the calibre of the descending colon was so contracted, that, when cut across, it appeared as if encircled by a ring of "a dull white, yet glistening fish-like substance," fibriform, and from half an inch to upwards of an inch in thickness.

Mr. Middlemore read a brief notice of the methods that have been used for the removal of capsular cataract, where the opaque capsule remains after absorption of the lens, for the purpose of introducing to the Section an instrument to facilitate the operation of extraction, without interfering with the transparent structures of the eye. The instrument consisted of a needle, accompanied by a small forceps, the former capable of being withdrawn, leaving the latter to be fixed on the opaque membrane, and then withdrawn through the sclerotic, through which the needle had been introduced.

In answer to a question from Prof. Macartney, Mr. Middlemore stated, that he had not yet practically proved the efficiency of the instrument.

Section F.—Statistics.

President—Mr. Henry Hallam.
Vice-Presidents—Sir Charles Lemon, Bart. M.P., Mr. G. R. Porter.
Secretaries—Messrs. Francis Clark, Rawson W. Rawson, Dr. W. Cook Taylor.
Committee—The Earl of Dartmouth, Rev. Thomas Arnold, D.D., Mr. G. Barker, Rev. J. Bradshaw, The Chevalier Bunsen, Messrs. J. Currie, W. Felkin, C. R. Fripp, W. R. Gregg, The Worshipful the Mayor, Messrs. J. Kingsley, W. Langton, Prof. Lipsius, Mr. E. W. Pendarves, M.P., Sir Oswald Mosley, Bart., Messrs. F. Scheer, T. Tooke, J. Wickenden.

Mr. Clarke read a paper entitled, 'Contributions to the Educational Statistics of Birmingham, by a Local Committee.'

Mr. Clarke stated, that a local committee had been formed to investigate the statistics of the borough of Birmingham, preparatory to the meeting of the British Association; but that in consequence of several unfortunate circumstances, and particularly the recent riots, which had engrossed the attention of the more active members, several heads of inquiry had been abandoned, and the information on others was meagre and imperfect. A report on the general state of Education in Birmingham, prepared by Mr. Wood, agent to the Manchester Statistical Society, was found to be imperfect, and was, in consequence, suppressed by the committee. The Educational Returns presented by Mr. Clarke comprised, 1. A return of the numbers, arrangement, and standing of the boys in the Free Grammar School of Edw. VI.; 2. A return from the Blue Coat School; 3. A return from the Park-street Charity School; 4. A return from the Deaf and Dumb School.—1. *King Edward's School:* The number of boys educated is 444. Branch schools are fast coming into operation in various parts of the borough, for giving the children in the lower grades of the Middle Class a sound English education. The present income is about 4,500*l.*; but in three years it will be doubled by the falling in of leases.—*The Blue Coat School*, for the education of children belonging to the Established Church. Income, 2,715*l.* annually. Boys educated, 143; girls, 63; total, 206.—*Protestant Dissenting School*, in Park-street, for girls only. Income, 615*l.* 6*s.* 9*d.* Girls educated, 46. Products of labour, 7*l.* 5*s.* 1*d.*—*School for the Deaf and Dumb:* The present numbers are, boys, 22; girls, 25; total 47,—of these 37 were born deaf and dumb, 10 became so at a very early age from illness. In five cases, other members of the same family were similarly afflicted.—To this report were appended some pathological and physiological remarks, from which it appeared that the imperfection of the senses most frequently occurs in the offspring of marriages between first cousins and other near relations.—*Literary and Scientific Institutions:* The Philosophical contains about 400, the Mechanics 450, and the Athenæum 300 members. To this paper were added, an account of Lynch's Trust, a charity for aged females; and an account of the number of inquests held during fourteen years. The average of inquests during the first seven years is 115, and during the last, only 170, though the population has nearly doubled, and the use of dangerous machinery has increased in a still greater proportion. The average of accidents from machinery, in the first period, was 26, and in the second, 37.

'Suggestions in favour of the systematic Collection of the Statistics of Agriculture,' by G. R. Porter.

If, (said Mr. Porter,) throughout the whole range of *material* interests that affect the well-being of a community, there be any one subject of greater importance than another, without doubt that subject is the adequate supply of food for the people; and yet this is a subject that, in England, which prides itself on being the country of practical men, has never hitherto been considered to any useful or practical end. The amount of ignorance on this subject is so great, that to this day the public does not possess any authentic document, from which we can learn even the quantity of land under cultivation in any county of England. The only information available for further calculation, is contained in the estimate of Mr. Couling, who gave evidence before a Committee of the House of Commons, which was appointed in 1827, to inquire into the subject of Emigration. Such an estimate, it is manifestly beyond the power of any individual to make with the requisite degree of accuracy. If even it be conceded that Mr. Couling's estimate was correct at the time it was offered, we must bear in mind that it was given more than twelve years ago, since which time we have added at least four millions to our numbers, and have therefore necessitated other important changes, which must influence the degree and proportions that might then have correctly designated the employment of the soil. Allowing, further, for the moment, that not only was the estimate correct, but that we had the means of rightly adjusting it to the altered circumstances of our population, we should still have acquired a very small, and only an initial part of the facts which are required, in order to arrive at any useful result. To this end it would be necessary to know, not only the extent of cultivated and improvable land within the kingdom, but also the various uses to which it is applied, and the proportion allotted to each branch of cultivation. We should have then to inform ourselves concerning the productiveness of the soil; the proportion of its produce required for reproduction; the number of each of the various kinds of animals reared and maintained; the rate of their increase; the proportion of each required for the adequate reproduction of its own kind; the number applicable to the sustenance of the people; the number of skins furnished for purposes of manufacture; the number of fleeces shorn: with various other particulars which it is not necessary to enumerate here. Neither would it be sufficient that these points should be ascertained once for all. We require to know not only whether the land is capable, under ordinary circumstances, of supplying the absolute wants of the population, but also whether or not the actual produce of each year, as it occurs, is sufficient to that end; and if it be not sufficient, then to ascertain the proportion in which it falls short of sufficiency. For these purposes, it is indispensable to have an organized machinery in constant action. The importance of accurately knowing the provision available for the sustenance of the people is not less than that of knowing the yearly produce of various articles of commerce which are used as accessaries in manufacturing processes. The condition of the crop of Indigo in Bengal is accurately communicated to the merchants of London at the earliest moment, and the information thus given has, through its influence upon the market price, an immediate effect in checking or promoting consumption. The like result attends on the collection of information concerning the growth of hops in this country; the extent of land devoted to the cultivation of that article being known through the records of the Excise Office, the department which is intrusted with the collection of the hop duty. In Belgium and Holland every kind of information connected with agriculture is obtained with the nicest accuracy. This duty is performed under the superintendence of a body of gentlemen, usually proprietors of estates, residing in different places, and who are elected in the respective provinces for purposes of local government, by the same persons who elect the members of the representative chambers. Having the requisite amount of local knowledge bearing upon the condition and circumstances of the several *communes* in their respective districts, they are able to prevent or to detect errors in the returns, which are made at certain periods according to forms prescribed for the purpose, by the farmers or occupiers of land. The result is every year the subject of a detailed report, which is printed under the authority of the governor of the province. A considerable amount of information on the subject of agricultural statistics is also collected in different parts of France. A statement was published in September 1838, by the Prefect of the Department of the Eure, containing a complete account of the agricultural produce of each subdivision of that department. In this statement may be found a separate account for each canton, giving its population and superficial area, the distribution of the soil, the nature of the crops, the breadth of land appropriated to each kind, the total produce, the average price of each description of produce, the quantity used for seed, and the consumption by the native population. There are also given, the extent of land appropriated to the growth of wood, and the quantity lying fallow, the number and value of different kinds of animals reared and

kept, the number slaughtered in the year, and the price of each kind of meat. In the course of a slight examination of this publication, comparing its results with such facts as we possess concerning the agriculture of our own country, some very striking differences appear, into the consideration of which it is not necessary to enter minutely on this occasion. It will, however, be interesting to state that the produce of wheat throughout the department is not equal, on the average, to quite 18¼ bushels to the English acre, and that the return obtained from the seed sown is not greater than sevenfold, results which may be pronounced by no means equal to those obtained by English farmers.*

The employment of the machinery necessary for the effective performance of the labour suggested must, in this kingdom, be attended by considerable expense. It cannot be expected that qualified persons would be willing to quit, or to neglect, other and perhaps gainful employments, for this purpose, except on the condition of being paid. It is probable that, in all parts of the country, gentlemen might be found, every way qualified, who would be willing to examine and control the returns when obtained; but those to whom the first task of collection is assigned must be paid. The number of parishes in the United Kingdom exceeds 14,000, and for each one of these, with the exception of town parishes, a paid agent must be employed. Then, too, the whole body of information, when collected, must be put into form by some competent person, who would also require payment for his service. To provide for the complete and correct performance of the work, might thus require an annual outlay of from 20,000*l.* to 30,000*l.*, a large sum unquestionably, and it is necessary, therefore, to mention some of the benefits which would flow from the possession of accurate information upon this subject. It is now well known, that the harvest of 1837 was deficient to so great a degree, that before the produce of 1838 was harvested, the great superabundance of the two preceding years was all consumed, and the stock of grain throughout the kingdom, whether in the hands of farmers or dealers, was more nearly exhausted than at any other period in modern times; so that if we could not have had recourse to foreign wheat then warehoused in our ports, we must have suffered most severely before the produce of 1838 could have been brought to market. So entirely, however, was the public misinformed on the subject, that instead of a reasonable advance in price sufficient to give warning of the fact, and so to check the consumption, which, as regards the most costly grain, wheat, had, it is well known, long been going on with unwonted profusion, the markets fell progressively during the months of August, September, and October, 1837, from 60*s.* 1*d.* to 51*s.* per quarter; and it was not until the middle of the following May that the weekly average was again as high as it had been immediately before the harvest of 1837. By the third week in August, 1838, the weekly average price had advanced beyond the rate at which foreign wheat is admitted to consumption on payment of the nominal duty of one shilling per quarter. It would not be possible to calculate, with any tolerable accuracy, the loss which this country sustained through the want of information thus shown to have prevailed with regard to only one year's operations; but we can have no hesitation in believing that its amount must have been at least sufficient, if employed at interest, to provide in perpetuity for every expense that could accompany the most completely organized machinery for collecting agricultural statistics throughout the United Kingdom. If the very considerable purchases of foreign wheat which were suddenly made during the autumn of 1838, in almost every market of Europe, occasioning the prices to be driven up to an exorbitant height, had been made progressively and quietly, without exciting a spirit of speculation, no one can doubt that the saving would have been sufficiently great to have borne out the assertion just hazarded. Whether the quality of suddenness given to the purchases made by us in foreign corn markets in the autumn of 1838, may not have occasioned an aggravation of the evil under which the money dealings of the country have since been suffering, is a question as to which different opinions may be entertained. It does not appear unreasonable to conceive that in proportion as those purchases could have been made to approximate to the conditions of ordinary commercial dealings, so would the derangements which they were calculated to occasion, have been diminished, or, to speak more correctly, they would not have been experienced. The gold which has left our shores, in payment for corn, will doubtless find its way back in exchange for goods, when time shall have admitted of the requisite adjustment. If time —that important ingredient in most human transactions—had been allowed to enter more largely at first into the conditions of our recent corn trade, that adjustment would, to some extent at least, have accompanied its progress. Our corn-laws were intended, not as a mode of acquiring revenue, but wholly for regulation and protection. It may not, therefore, be right to insist very strongly on the sums which, under the circumstances just supposed, might have found their way to the Exchequer from this source in 1838, beyond what were actually collected. It may, however, be interesting to state, that if the average rate of duty received on the foreign wheat and wheat-flour brought into consumption in 1838, had been the same as was received on that admitted in 1837, the Exchequer would have benefited by no less a sum than 2,156,596*l.* beyond the money actually received,—and one-fourth part of this sum, if employed at interest, would yield a revenue sufficient for making the requisite statistical inquiries concerning agriculture throughout the kingdom. It would be taking a very narrow view of the subject, to limit the advantage of knowledge concerning it, to the amount of saving that might be effected under a recurrence of such circumstances as have attended the deficient harvests of 1837 and 1838. The natural, the necessary tendency of any systematic and comprehensive inquiry, such as might be made by public agents, acting under legislative sanction, would be to place the nation as far as possible, and for a long series of years, beyond the probability of scarcity. It is well known by everybody who has made any extensive inquiries concerning the state of agriculture as a science, in various parts of the kingdom, that the improvements made in some counties have not been hitherto followed in others; and it has been stated, in illustration of this fact, "that if all England were cultivated as well as the counties of Northumberland and Lincoln, it would produce more than double the quantity of food that is now obtained." To what can this be attributed, unless it be to the want of information, such as the public agents would collect in every county, and which could not fail to interest deeply every cultivator? Farmers have often been accused of an unwise preference for the plans and processes employed by their ancestors, and their pertinacity in this respect has been contrasted with the rapidity which accompanies the march of improvement in mechanical and manufacturing processes. The reason for such difference is obvious. Manufacturers residing in towns are brought necessarily and constantly into collision; new inventions are continually brought under their notice, so that their results can be tested and judged with the greatest facility and accuracy. Practical agriculturists, on the contrary, live apart; they come together but rarely, and have not that degree of acquaintanceship the one with the other, which leads them even when they meet, to the interchange of professional experience, and if the result of a successful agricultural experiment should at any time reach their ears, they cannot be certain that all the circumstances connected with its prosecution have been faithfully detailed. The tendency of statistical inquiries such as would be carried out by the means already described, would be in a great degree to remove the disadvantage under which the farmer now suffers. Authority, resting not upon a single experiment only, but upon the successful practice of hundreds of men placed in similar circumstances with himself, must have the authority of undoubted facts, and the farmer would be compelled to adopt whatever should come thus recommended, with an alacrity equal to that which we see exhibited by the manufacturer. It may perhaps be feared, that if the public should acquire a knowledge of any peculiar advantages, those by whom they are enjoyed would be exposed to have them invaded. The agricultural is in this country what is called, a *protected* interest. Our cultivators consequently appear desirous of excluding inquiries which they think might have the effect of weakening their claim to the continuance of that protection. This fear is altogether chimerical. In Belgium, where the utmost publicity is given to every fact connected with the agriculture of the country, it has never been pretended that any improper advantage has been taken of that publicity. The knowledge which it appears to be so desirable to obtain with reference to the whole kingdom, is already easily procurable with regard to every individual farm, by any person whose interest may incite him to the task. The landlord, who is interested in extracting a due proportion of the produce of the farm under the name of rent, cannot find much difficulty in correctly estimating that produce. A like facility attends the operations of the tithe proctor and of the parochial tax-gatherer. The knowledge is, in fact, already procurable by every one who can turn it to the disadvantage of the farmer; all that is wanted is to extend the information, so that the farmer himself may be placed in a condition to profit from its possession, and thus to add to the sum of the general prosperity.

* The produce of barley, not much of which is grown in the department, does not exceed 17 bushels per acre; of oats not quite 20½ bushels per acre are obtained.

Several objections were made to the proposed plan, and it was urged that the acquisition of such knowledge would involve a greater amount of expenditure than was even contemplated by Mr. Porter; and it was doubted whether machinery for collecting the information existed. The general opinion, however, seemed to be in favour of Mr. Porter's plan.

'A Report on the State of the Working Classes in part of Rutlandshire, by the Manchester Statistical Society,' was read by Mr. W. R. Gregg.

The Statistical Society of Manchester having completed and published an inquiry into the condition of the working classes in several large manufacturing towns in the north of England, were desirous of obtaining similar information with regard to some population differing in character and circumstances from those which had been previously examined. For this purpose they selected three parishes in Rutlandshire, (a purely agricultural county,) which they conceive may be assumed to afford a fair sample of the whole. The information thus obtained they have arranged in a series of tables, of which we shall briefly enumerate the most striking results. The parish of Branstown, which lies on the western side of the county, contiguous to Leicestershire, contains about 1,400 acres, more than three-fourths of which are pasture land. It has a population of 102 families, comprising 425 individuals, but there is no resident clergyman, and no resident landlord possessing any extensive property. The parishes of Egleton and Hambleton (which have been classed together,) are situated at a very short distance from Oakham, the county town, and contain about 2,400 acres, which are about equally divided between arable and pasture. The chief part of both these parishes is the property of Mr. Finch, who resides upon his estates, and has the character of a kind landlord. The population consists of 101 families, comprising 479 individuals. The first three tables presented to the Section related to the dwellings of the population; and the account of them on the whole was satisfactory. The houses are low, never exceeding two stories; many of them are thatched, and nearly all are built of stone. To each a garden is attached, which is generally of sufficient dimensions to supply the family with vegetables. Forty per cent. of the dwellings in Branstown, and 51 per cent. in Egleton and Hambleton, are reported to be well furnished, which was very nearly the proportion found in Manchester and Dukinfield. The proportions reported to be comfortable were:—

In Branstown	50 per cent.
Egleton and Hambleton	65 ,,
Manchester	72 ,,
Dukinfield	95 ,,

The general appearance of the interior of the houses indicated thrifty poverty, and instances of squalid misery were very rare. Thirty-one per cent. of the houses in Egleton and Hambleton contained four rooms, and only 17 per cent. in Branstown. In respect to sleeping accommodations:—

In Egleton, 14 per cent. of families have more than three persons in a bed.

Branstown,	19	ditto	ditto.
Dukinfield,	33	ditto	ditto.
Bury,	35	ditto	ditto.

The rent of houses is low:—

In Egleton, the average yearly rent is	£2	17	3
Branstown	3	0	0
Dukinfield	6	14	0
Manchester	7	11	8

The proportion of public-houses and beer-shops to the population is—

In Branstown....1 in 85.
Egleton .....1 in 240.

The average weekly wages of families vary from 9*s.* 7*d.* to 10*s.* 8*d.*, and about one-fourth of those under twenty-one are earning wages. The state of education in these parishes was rather satisfactory. The tables give the following results:—

	Per cent. can read.	Per cent. can write.
Branstown	75	44
Egleton, &c.	81	50
Manchester	50	Unknown.
Dukinfield	53	28

In Egleton, where there is a resident clergyman, the establishment has 82 per cent. of the population; and in Branstown, which is destitute of this advantage, only 73 per cent. In conclusion, the Committee report that their inquiries left a favourable impression as to the moral condition of these places. Swearing and drunkenness are far from common; and the general conduct of the people is marked by sobriety, frugality, and industry.

A desultory discussion arose, turning principally upon the difficulty of getting an accurate estimate of the wages of labour.

Section G.—MECHANICAL SCIENCE.

President—Professor Willis.
Vice-Presidents—Mr. Robert Stephenson, Dr. Lardner, Mr. G. Rennie.
Secretaries—Messrs. W. Carpmael, W. Hawkes, T. Webster.
Committee—Mr. C. Askin, Dr. Brown, Messrs. J. C. Birkinshaw, C. H. Capper, G. Cottam, Dr. Church, Messrs. P. Clare, — Dockray, E. Dent, W. Fairbairn, — Good, H. Gilbert, J. Glynn, E. Hodgkinson, J. I. Hawkins, A. Liddel, Capt. Moorsom, Messrs. J. Nasmyth, F. Osler, J. Parkes, Westley Richards, J. Scott Russell, R. Roberts, J. Reynolds, J. Taylor, Capt. Vetch, R.E., Messrs. E. Woods, — Whitworth.

Dr. Lardner, V.P. in the chair.

Mr. J. I. Hawkins made a communication on paving roads and streets with blocks of wood, placed with the grain in a vertical position. The subject, he observed, has latterly become one of considerable interest. Although seven patents have been taken out in this country within less than twelve months, there is no specimen of the pavement calculated to afford the means of forming a sound public opinion on the subject. He had attentively watched, from 1827 to 1831, the effect of much travelling over wooden pavement, well executed, in the principal thoroughfare of Vienna, and observed that it appeared to wear away less than any other kind of paving material whatever. In this opinion he was confirmed by inquiries which he made relative to the condition of a piece of wooden pavement laid about three years in the Broad-way of New York; and he had been informed by one gentleman that he saw a stone of near 20 tons weight drawn on a carriage over it without appearing to make the least impression. From these circumstances he considered that roads formed of sound wood, with the grain vertical, might be made so even as to constitute a sort of universal railway, on which carriages might be drawn by a small proportion of horse-power, and on which steam-carriages might run as safely and almost as fast as on railways. The directions to be attended to in the formation of efficient and durable roads on this principle, which the author gave, were comprised under the following heads. 1. The wood must be chosen from the heart of sound trees. Larch and other resinous firs offer excellent materials at moderate prices. 2. The blocks, which are to be laid contiguously, must be cut to an exact gauge, so as to fit closely and evenly together, and no block must be higher than another. 3. The depth of the blocks should be at least that of a breadth and a half, a firm lateral support being found necessary to stability. Each block, when rectangular, is supported by four others, and when formed into hexagonal prisms, which appears to be preferred, each block is supported by the six surrounding ones. The hexagonal prism being found to afford the greatest quantity of wood from a tree when the diameter of the prism is as large as can be cut out of the whole diameter of the tree, that figure is generally adopted, and has been fairly tested by experience. 4. The blocks must be laid on a bed firmly made with gravel, shingle, hard rubbish, or other material, well rammed down, and made even, previously to laying the blocks. 5. A thin layer of only half an inch of fine gravel must be spread evenly over the levelled surface at the time of laying the blocks, to favour their adjustment. 6. The blocks must be laid so as to present an even upper surface before they are rammed, in order that the ultimate making them level shall not depend so much on the effects of the rammer as on the evenness of the bed. It is essential that the blocks be cut from dry wood, and used soon after being cut, lest their figure vary by warping.

In reply to a question by Mr. Carpmael, it was stated by Mr. Hawkins that there were not sufficient results on which to decide with respect to the durability of different woods.

Mr. John Taylor stated that at some of the earlier meetings of the Association it was deemed advisable to make a series of inquiries relative to the duty of Cornish engines, the nature of which was not much understood. For many years the largest proprietors had directed their attention to the subject, and appointed competent persons to ascertain how many strokes per minute, and how much coal had been used. Ingenious machinery had been invented for this purpose, and registers had been kept since 1815, which now formed an important and valuable historical record. For the reduction of the accumulated observations a grant of money had been made by the Association, and placed under the direction of himself and Mr. Rennie, and which sum had been awarded by them to Messrs. Lean, whose work was now published.

'On the most Economical Proportion of Power to Tonnage in Steam Vessels,' by Mr. Scott Russell.

It is a subject of anxious inquiry with every proprietor and constructor of steam vessels, when about to construct a new steam vessel for a given station, What is the best amount of power to place in my ship, so as to accomplish in the highest degree economy, rapidity, and regularity? The inquiry is one accompanied with difficulties. If, on the one hand, a large cargo is desired, which is generally the case, the power of the engine is made small, for the purpose of occupying small bulk, and consuming little coal; and although less velocity is then acquired than with greater power, still less fuel is consumed with a small engine than a large one, and thus it is supposed that greater economy is effected. This maxim, of a small proportion of power to tonnage, is one on which many companies have long continued to act. In other cases, where velocity is absolutely required, a much larger proportion of power has been employed, and, of course, by a large power there is a much greater consumption of fuel than a small one in a given time; and not only so, but it is well known that this additional consumption is much greater than in the proportion of the velocity gained; so much so, that a consumption of four times as much fuel will not give more than about double the velocity. Thus, it has appeared that the use of very great powers, and great expenditure of fuel, has been made with only a very slight increase of velocity. All this, the usual reasoning on the subject, goes to prove the value of employing a small proportion of power to tonnage for economy. The advantage of low powers and of low velocities, in point of economy, would thus appear to be established: but this apparent advantage in theory has not been realized; on the contrary, it may be recollected, that at the Bristol Meeting of the Association, Dr. Lardner remarked, that in a case which had come under his own knowledge, where the power had become gradually increased in the same vessel, and it was found that while the vessel was increased in speed, the consumption of fuel on the whole had diminished. This was a fact which Dr. Lardner attributed to Mr. Russell's Wave principle. Mr. Russell had himself attributed it at that time to the same cause, but he had afterwards considered the subject worthy of further examination. He examined the books of the expenditure of fuel in the steam vessels of several companies, and found that they were aware that they had, on the whole, saved money by using high powers of steam and high velocities, instead of low ones. This result he would not in the circumstances attribute to his wave principle; and he had, therefore, examined it carefully, and had arrived at a very remarkable general result, which appeared to him quite new, and to be of very great value at the present moment, when such important interests were involved in the successful extension of deep sea navigation. The general principle at which Mr. Russell had arrived, was this:—*That in a voyage by a steam vessel in the open sea, exposed, of course, to adverse winds, there is a certain high velocity and high portion of power, which may be accomplished with less expenditure of fuel and of room, than at a lower speed with less power.* This principle he then proceeded to prove, and to illustrate by the following case, in which the same vessel is taken with different powers of engine, and the result, as regards expenditure of fuel, determined first arithmetically, and then by a general formula, which will enable any one to determine any particular case.

Fair Weather.

1,200 tons, 400 horse-power, 9 miles an hour, 216 miles per day, 1 ton of coals per hour.—2,160 miles in 10 days, 240 tons of coal.

1,200 tons, 500 horse-power, 10 miles an hour, 240 miles per day, 1¼ ton of coal per hour.—2,160 miles in 9 days, 270 tons of coal.

Adverse Weather.

1,200 tons, 400 horse-power, 5 miles per hour, 120 miles per day, 1 ton of coals per hour.—2,160 miles in 18 days, 436 tons of coal.

1,200 tons, 500 horse-power, 6¾ miles per hour, 162 miles per day, 1½ ton of coals per hour.—2,160 miles in 13 1-5th days, 395 tons of coal.

General Formula.—Let v represent the velocity of a given steam vessel in a favourable voyage; v' the same vessel in an unfavourable voyage; v'' a vessel, higher power, favourable voyage; v''' unfavourable voyage; p the power of the former vessel; p' latter vessel:—

$$\therefore \sqrt{v''' - (v - v')} = v''' = \sqrt{v} \sqrt{\frac{p'}{p} - (v - v')}$$

$$v\frac{p'}{p} = \sqrt{v'' - (v - v')}{v'}$$

in the case of equal expense, whence the highest proportion of power that will be economical in fuel may be at once obtained.

Dr. Lardner remarked that the theory of Mr. Russell was quite new to him; but, after the exposition which he had heard, he was satisfied of its accuracy.—Mr. Russell observed that there was another important circumstance to which he had not alluded, and in which great advantage might be derived from the application of a high power: thus the owner of a vessel which usually accomplishes her voyage in fifteen days, but, owing to storms or adverse winds, occasionally takes twenty, must so regulate her stated periods of departure as to allow for these adverse circumstances; and four or five such vessels may be required to keep up a regular communication between places for which three or four would be sufficient, if, by increased power, the average voyage were reduced to ten, and the adverse to fourteen days; and thus a large capital would be saved, as well as more voyages accomplished. The principles which he had developed might also, he thought, be applied advantageously under many other circumstances,—such as in mines subject to uncertain in-rushings of water, &c. In reply to a question, Mr. Russell stated that his principle was only true in reference to vessels making long voyages, for, where not exposed to adverse contingencies of wind and weather, a greater additional power would be merely an additional expense.

Dr. Lardner described an apparatus which he had constructed for use in working railways. From various circumstances which he had observed, he was of opinion, that the rails were often not so perfect as they ought to be, and he had hence thought it advisable to make some experiments to ascertain their rate of deflection, his object being to form an idea of their relative merit of surfaces. He made use of a truck, with wheels without flanges and perfectly cylindrical, and on which a platform was placed. An iron tube crossed this, terminating at each end at right angles, and into this was introduced mercury, so that it was in fact a mercurial level. On placing this along the lines of railway the columns were found to go up and down, an effect which was not produced by oscillation. Into these columns was introduced a piston rod, to the top of which a pencil was attached, which on any incorrectness of the line described a curve on a sheet of paper, the ordinate of which gave the variation of the rail. He had tried this on several parts of a line, where it gave a continual variation in the curve, amounting even from three to five inches. The instrument was checked so as to show that this curve was the real representation of the line, and, being simple and easily applied, would no doubt be found useful to contractors on new lines of rails.

GENERAL MEETING.—MONDAY EVENING.

This Meeting took place in the magnificent Town Hall, the Rev. W. Vernon Harcourt, President, in the chair, supported by the Marquis of Northampton, the Earl of Dartmouth, &c.

Mr. Murchison, the General Secretary, stated briefly that at the special request of the Council, the President had drawn up a discourse for the occasion. To the exertions of Mr. Harcourt, and to the luminous views which he had taken of the objects of the Association, delivered at its first meeting in York, its success was mainly attributable: and the members had justly recognized his zeal and services, by electing him President.

ADDRESS OF THE PRESIDENT.

A few weeks since I bade farewell to one whose friendship I owe to this Association, setting forth on an enterprise, full of labour and hazard, but full also of such visions of glory, and so brilliant a prospect of scientific conquests, that for a mind combining the high aims of the philosopher with the intrepidity of the sailor, no danger, no difficulty, no inconvenience seemed to exist, even in those regions where

> Famine guards the solitary coast,
> And winter barricades the realms of frost.

We sat down, Gentlemen, before his chart of the Southern Seas, and the unapproached pole of the earth; he showed me his intended track—he pointed out the happy coincidence of the recent discovery (one of the debts which science owes to commerce) of two small islands in those seas; he put his finger upon the spot which theory assigns for the magnetic pole of verticity, corresponding to that which he had himself discovered in the opposite hemisphere, and so situated, intermediately between the two newly-discovered insular stations, that should he not reach the pole itself, they would enable him to verify or correct the theory—and again on the spot where his course would cross the point of maximum intensity which the same theory involves. We next reviewed the places at which he is commissioned to plant, on his way, three magnetical and meteorological observatories, St. Helena, the Cape, and Van Diemen's Land, and those at which he himself especially wished to observe, at Kerguelen's Land, New Zealand, and other stations on the land and ice; and we talked of all these as part only of a system of observations simultaneous or combined, stretching from one side of the earth to the other, undertaken or promised, through the whole extent of the British empire, from Montreal to Madras, and blending in co-operation with chains of observatories established, or on the point of being established, by other nations in the four quarters of the world.

I confess, Gentlemen, I felt, as one of the white and bright moments of life, such a conversation, at such a moment, with a man, of whom, as he is no longer with us, I may venture to say, that he is worthy of being employed on so glorious a service.

When I had bidden him adieu, I had leisure to reflect on the possible consequences of his expedition, and the plan of which it forms a part. The problem of terrestrial magnetism solved—first, the laws of the changes of its elements detected, their constant parts determined, and the whole proved to coincide with a theory based on *a legitimate representation of known facts*—then the lines of its force and direction truly drawn, the deviations predicted, and the corrections supplied—in the immediate view of practical consequences, our ships finding in their compass-needles a more unfailing guide than in the fragile time-piece or the cloudy sky—in the distant horizon of higher and yet more fruitful speculation, the *true cause* of the phenomena—and therein perhaps a completion of what Newton began—a revelation of new cosmical laws—a discovery of the nature and connexion of imponderable forces—all these the possible results of approaching the heights of theory on what may prove to be their most accessible and measurable side.

Afterwards I thought of the causes which had conduced to this grand undertaking, which had prompted the British government to seek these laurels, and cull these fruits of peace, to fit out the most important and the best appointed scientific expedition which ever sailed from the ports of England. Well, Gentlemen, were the government both prompted and seconded by the science of the country. I saw the apartments of the Royal Society moved by a spirit of energy and zeal; its most distinguished members sacrificing personal considerations, and postponing individual to public objects—Committees meeting and corresponding, to perfect the instruments of observation, and prepare the plans for observing—distant members, at Dublin and Woolwich, deputed to instruct the observers. It seemed as if the days of Wallis, and Wilkins, and Wren, and Boyle, and Evelyn were revived; and whom did I recognize, Gentlemen, among those who were thus zealously and effectively employed? Their faces were familiar to me, they were the same men who first mooted the subject, and discussed it together at the meetings of this Association:—the same who went from you to call the national attention to it, had added to that call all the influence, and all the efficacy of the Royal Society. The individual, again, appointed to command the expedition, and conduct the observations, and those also who were selected to instruct the other observers, who were they? they were not only taken from the ranks of the Association, but they had perfected the instruments of observation, and gained additional experience in observing, during their co-operative labours in its service. When Captain James Ross and Major Sabine, and Profs. Lloyd and Phillips, and Mr. Fox, were engaged in ascertaining the curves of the magnetic elements across the British islands, with unexampled completeness, they performed a national work important in itself, but still more important as leading on to greater undertakings. But lastly, Gentlemen, whence proceeded that theory which it is the highest object of all this philosophical energy, and all this national liberality to put to the test—the first profound attempt to bring the magnetism of the earth under the dominion of calculation? Let the illustrious author of it speak for himself: "Several years ago," says Gauss, "I repeatedly began attempts of this kind, from all of which the great inadequacy of the data at my command forced me to desist." "The appearance of Sabine's map of the total intensity, in the 7th Report of the British Association for the advancement of science, has stimulated me to undertake and complete a new attempt"—an attempt, Gentlemen, which, whether we consider the importance of its results, or the labour and the strength expended upon it by this great mathematician, reflects high credit on the author of the map, which provided for such a theory numerical expressions, and does honour to the Institution which was in any the least degree instrumental to its production.

In all that I have been saying, Gentlemen, I have been desirous of pointing to that spirit of co-operation which our meetings have called forth in this country; we see among us now many fraternities of fellow-labourers working for a common cause, on a common plan, with a perfect mutual understanding: this is the only means of advancing those great branches of knowledge in which space is a necessary element, and it is the best security for a constant progress in all. Science, in a country where every man labours alone, resembles those stars which are seen from time to time to "pale their ineffectual fires;" but there need be no fear of its decline, there can be no check in its advance, when it depends not on the prowess of any single arm, but on the force of its numbers and the order of its array.

The system of your meetings, Gentlemen, has brought together things which ought never to be disjoined—the principles of science, with their application to human use. After gathering your first members from our ancient schools of learning, you passed to the marts of commerce, and are now come to the heart of the manufactures of England, and look round on all the resources and creations of mechanical art. The theorist and mechanician here meet together to the mutual advantage of both; witness on the one part the instrument now working in the Philosophical Institution of this town, and almost supplying the place of a constant observer, which is about to measure the force of the wind at every instant of time, at St. Helena, the Cape of Good Hope, Van Diemen's Land, and near the Southern Pole. On the other hand, I may mention an anecdote which shows by how circuitous a route art has sometimes been driven to seek the aid of science. During the war between France and England, a Frenchman brought with him the discovery of a great chemical philosopher in Paris to barter for a secret of the English manufactures; not finding in Lancashire the person he sought, he left a message, returned to London, and was imprisoned under the Alien Act; to prison, however, the English manufacturer followed him obtained his secret, and his liberation, made his own fortune, and enriched his country.

But need I go further than the immediate vicinity of this town for an instance, the most striking on record, of the mighty influence which the introduction of a new principle in science can exercise on all the arts of life; the history of the improvement of the steam-engine by Watt finely illustrates this truth. In the eulogium of that great man lately published,* the Secretary of the French Institute has justly and eloquently displayed, by this memorable example, the power which resides in the unaided genius, industry, and patience, of a single individual, applying his mind to the fruitful application of a scientific truth, and the incalculable extent to which he may promote the welfare of his country, and benefit the whole family of mankind. He has taught us also to reflect "in what an humble condition of life those projects were elaborated, which were destined to carry the British nation to a degree of power hitherto unheard of."

But whilst I refer you to this volume, Gentlemen, for an admirable exposition of important truths, I feel myself called upon to state, that the zeal of M. Arago has carried him too far, when it has tempted him to transfer to Watt those laurels which both truth and time have fixed upon the brow of Cavendish.

It is far from my views, Gentlemen, to draw any comparison between two illustrious names, of which one stands as high in the discovery of natural facts, as the other does in their useful application; but let us hold a just and even balance between genius that rises superior to the pressure of circumstances, and that which reaches to at least equal intellectual heights, unseduced by rank and riches. The Secretary of the Institute has not confined himself to taking from Cavendish the honour of his discoveries, but has cast a cloud of suspicion on his veracity and good faith: he has, in fact, imputed to him, the claiming discoveries and conclusions which he borrowed from others, of inducing the Secretary of the Royal Society to aid in the fraud, and even causing the very printers of the Transactions to antedate the presentation-copies of his paper.

Yet this, Gentlemen, is the man to whom, at his death in 1810, one who knew and was competent to speak of him, bore the following testimony:—"Of all the philosophers of the present age," said Davy, "Mr. Cavendish combined the greatest depth of mathematical knowledge with delicacy and precision, in the methods of experimental research. It might be said of him, what, perhaps, can hardly be said of any other, that whatever he has done has been perfect at the moment of its production: his processes were all of a finished nature; executed by the hand of a master, they required no correction, and, though many of them were performed in the very infancy of chemical knowledge, yet their accuracy and their beauty have remained unimpaired, amidst the progress of discovery, and their merits have been illustrated by discussion, and exalted by time. In general, the most common motives which induce men to study, is the love of distinction and glory, or the desire of power; and we have no right to object to these motives; but it ought to be mentioned, in estimating the character of Mr. Cavendish, that his grand stimulus to exertion was evidently the love of truth and knowledge: unambitious, unassuming, it was often with difficulty that he was persuaded to bring forward his important discoveries. He disliked notoriety; he was, as it were, fearful of the voice of Fame; his labours are recorded with the greatest simplicity, and in the fewest possible words, without parade or apology; and it seemed as if, in publication, he was performing, not what was a duty to himself, but a duty to the public."—"Since the death of Newton," he concludes, "England has sustained no scientific loss so great as that of Cavendish

* Annuaire.

—his name will be an object of more veneration in future ages, than at the present moment—though it was unknown in the busy scenes of life, or in the popular discussions of the day, it will remain illustrious in the annals of science, which are as unperishable as that nature to which they belong—it will be an immortal honour to his house, to his age, and to his country."

Alas! Gentlemen, for human predictions and posthumous fame! Who could have foreseen that, ere thirty years had elapsed, so opposite a view of the labours and character of this philosopher should have proceeded from one of the most enlightened of his successors? For the sake, then, of justice, and because there is no page in the history of experimental philosophy more instructive than that to which this question carries us back, I now ask permission to give equal publicity to a different view, and to offer such a sketch of the great chemical discovery of the composition of water, as may perhaps help to elucidate the truth.

According to the statements of this publication, the first person who proved that the water produced is equal in weight to the gases consumed, was Dr. Priestley; the first person who drew the conclusion that water is composed of oxygen and hydrogen, was Watt. Now, the first of these statements has not only no real foundation, but is contradicted by the repeated assertions of Priestley himself, who constantly maintained, that he had never found the weight of the water equal to the sum of the gases. The second, Gentlemen, has no foundation, except in the licence which M. Arago has used, of quoting the words of Watt otherwise than they really stand. Nor can there be a stronger instance of the inconvenience of such translations, than the difference of meaning and value in the words thus substituted for each other—*hydrogen*, for example, put for *phlogiston*.

What is it, Gentlemen, that gives importance to this discovery in the history of science? Not, as it has been too popularly stated, that water thenceforward ranked no longer among the elements. The prejudice of that epoch was, not to regard compound substances as simple, but to consider undecompounded substances as compound. The hypothesis, that a principle called Phlogiston entered into the composition of a great variety of bodies which we now consider simple, had infected the whole of chemistry. This hypothesis, at first but a conjecture to explain the phenomena of combustion, gradually made itself a coat of patchwork out of the successive discoveries of half a century, and arrived at playing as many feats in philosophy, as the harlequin in a pantomime. In the very paper of Watt's on which this claim is founded, we find, first, inflammable gas, then charcoal, then sulphur, then nitrogen, to be all different forms of the same phlogiston, united with a minute portion of different bases—we find it combining with oxygen, in one proportion, to form carbonic acid, in another, nitrogen, in another, water. Its affinities with these bases, and with all the metals, had been determined by Bergman, as well as the relative weights in which it entered into composition: and to complete all, the year before the publication of Cavendish's experiments, Kirwan had proceeded to give a table of the *absolute* weights—had computed that fourteen cubic inches of nitrous air contain 0.938 grains of phlogiston, and had actually deduced a law for these weights, corresponding with the specific gravities of the metals.

You will easily conceive, Gentlemen, the effect on a purely experimental science of such a hypothesis as this, and you must add the effect of other hypotheses, equally prevalent, which bestowed similar chemical affinities on the principles of light and heat; thus you find the ideas of Watt respecting the composition of water connected with, and springing out of the idea that it was reconvertible, not simply into phlogiston and dephlogisticated air, but, by an intimate union of the latter with the principle of heat, into phlogiston and atmospheric air. By such fancies as these, the best chemists of the day were misled, not only as to the direction of their labours, but even the results of their experiments. But in Cavendish's celebrated inquiry into the causes whereby air suffers diminution in a variety of processes then termed phlogistic, it is well worthy of remark how steadily he moves on from truth to truth, on every point on which experiments afforded ground for reasoning, unfettered by the complexity of the phlogistic theory; and it is equally remarkable, how loose he sits to the favourite hypothesis to which the rest of his countrymen clung with such persevering tenacity. He—first of all his cotemporaries—did justice to the rival theory recently proposed by Lavoisier, and weighed it in equal scales before the public eye. He alone seemed to understand, as it became a disciple of the school of Newton, the true use of an hypothesis: he valued neither system otherwise than as an expression of facts, or as a guide to future inquiry. He took these opposite hypotheses, and retrenched their superfluities; he pared off from both their theories of combustion, and their affinities of imponderable for ponderable matter, as complicating chemical with physical considerations, and he then corrected and adjusted them with admirable skill to the actual phenomena, not bending the facts to the theory, but adapting the theory to the facts.

Allow me to give you an instance of this adaptation. Cavendish has represented the constitution of water under the two suppositions, either as oxygen combined with hydrogen, or as dephlogisticated air combined, not with phlogiston, as Priestley or Kirwan would have said, had they admitted the fact, but with phlogiston and water. You will find the reason of this distinction, Gentlemen, by looking into the works of Priestley. This philosopher had stated, that he had obtained inflammable gas by the simple action of the burning lens on pure iron and charcoal, and drawn the consequence, that iron and charcoal were composed of phlogiston united to the bases of these bodies, and that inflammable gas was pure phlogiston. "I had no suspicion," he says,* "that water was any part of inflammable air;" "yet that water in great quantities is sometimes produced from burning inflammable and dephlogisticated air, seemed to be evident from the experiments of Mr. Cavendish and M. Lavoisier. I have also frequently collected considerable quantities of water in this way, though never quite so much as the two kinds of air decomposed." "Afterwards, seeing much water produced in some experiments in which inflammable air was decomposed, I was particularly led to reflect on the relation which they bore to each other, and especially Mr. Cavendish's ideas on the subject. He had told me, notwithstanding my former experiments, from which I had concluded that inflammable air was pure phlogiston, he was persuaded that water was essential to the production of it, and even entered into it as a constituent principle. At that time I did not perceive the force of the arguments which he stated to me, especially as, in the experiments with charcoal, I totally dispersed any quantity of it with a burning lens, *in vacuo*, and thereby filled my receiver with nothing but inflammable air. I had no suspicion that the wet leather on which my receiver stood could have influence in the case while the piece of charcoal was subject to the intense heat of the lens and placed several inches above the leather. I had also procured inflammable air from charcoal in a glazed earthern retort two whole days successively, in which it had given inflammable air without intermission: also iron filings in a gun-barrel, and a gun-barrel itself had always given inflammable air whenever I tried the experiment. "But, my attention being now fully awake to the subject, I found that the circumstances above mentioned had actually misled me." "Being thus apprised of the influence of unperceived moisture in the production of inflammable air, and willing to ascertain it to my perfect satisfaction, I began with filling a gun-barrel with iron filings in their common state, without taking any precaution to dry them, and found that they gave air as they had been used to do;" "at length, however, the production of inflammable air from the gun-barrel ceased, but on putting water into it the air was produced again; and a few repetitions of the experiment fully satisfied me that I had been too precipitate in concluding that inflammable air is pure phlogiston." Dr. Priestley afterwards gives an account (Phil. Trans. 1785,) of his repeating Lavoisier's celebrated experiment, in which the decomposition of water was proved by passing steam through an iron tube. "I was determined," he says, "to repeat the process with all the attention I could give to it, but I should not have done this with so much advantage if I had not had the assistance of Mr. Watt, who always thought that M. Lavoisier's experiments by no means favoured the conclusion that he drew from them. As to myself, I was for a long time of opinion that his (Lavoisier's) conclusion was just, and that the inflammable air was really furnished by the water being decomposed in the process; but though I continued to be of this opinion for some time, the frequent repetition of the experiments, with the light which Mr. Watt's observations threw upon them, satisfied me at length, that the inflammable air came from the charcoal or the iron"—that is to say, the phlogiston came from the iron or charcoal, and uniting with water, formed inflammable air.

Here we have a clue to the singular inconsistency of Watt's paper in making inflammable gas to be pure phlogiston, in one sentence, and phlogiston and water in another; in the interval which occurred between the writing of the two letters amalgamated in that paper, Priestley, under the correction of Cavendish, had repeated his experiments, and revised his opinion; Watt also had changed his opinion; but he was willing, nevertheless, to let a double speculation take its chance. It is evident that neither he nor Priestley had any greater claim to a clear expression of the constituent elements of water than they had to the discovery and proof of its composition.

Lavoisier was the first to introduce into chemistry a juster language and a safer method of reasoning; he caught sight of a principle which has been since laid down by Davy as a general proposition, and has cleared the way for many discoveries, the principle that every body is to be reasoned about as simple till it has been proved by direct evidence to be compound. To Cavendish, trained in the rules of demonstration, and gifted with a sagacity and clearness of conception beyond his fellows, hypothetical thoughts and expressions were no stumbling-block; and he seems therefore not to have felt how great an obstacle they present to the general movement of science as it floats upon the tide of a thousand understandings.

If the question then be, who reformed the expressions and logic of chemistry, or who furnished the simple terms in which we now state the elements of water? the answer is Lavoisier—but if it be, who discovered and unfolded the facts on which that reformation mainly relied? who detected and proved the composition of water, and deduced the train of corollaries which flowed from it? the answer is, Cavendish. The discovery was not one of those which was within every man's reach, especially in an age of loose experiment and inconclusive reasoning: it was one which could never have been made, but by a strict appreciation of quantities, and a careful elimination of the sources of error: it formed part of the solution of the difficult problem, which in 1780 occupied the attention of all the chemists in Europe—the cause of the diminution of atmospheric air, in five several cases—in the passage through it of the electric spark, in its burning with hydrogen, in its contact with nitrous gas, in respiration, in the inflammation of phosphorus and sulphur, and in the calcination of metals. All these phenomena had been accounted for by a supposed reunion of phlogiston with the dephlogisticated part of the air, and the consequent formation and absorption of carbonic acid; Lavoisier stood alone in attributing the calcination of metals to its true cause. When Cavendish took up the problem, he began by proving that no carbonic acid was necessarily produced in any of these processes: and then, Gentlemen, he turned to use a well imagined but incorrect experiment of an inhabitant of this town, (Mr. Warltire), and he made it the basis of a series of analytic and synthetic researches, unequalled then, and never since surpassed, by which he demonstrated incidentally, and as a means of arriving at the solution of his problem, 1st. The quantitative composition of the atmosphere; 2nd. The combination of hydrogen with oxygen and the quantitative composition of water; 3rd. The chemical union of nitrogen with oxygen and the constitution of nitric acid. From these experimental data he deduced the true causes of the diminution of the air in the burning of hydrogen, and in the processes of Eudiometry, and respiration; he adopted Lavoisier's conclusion respecting the burning of metals and other inflammable matters, and correcting his premature generalizations, gave the true account of the composition of the

* Priestley on Air, Ed. 1790, Part 3. Sect. 4.

nitrates of potash and mercury; finally, he explained the constitution of vegetable substances, and the origin of the oxygen which they exhale.

Such, Gentlemen, were the splendid results of this investigation, such the reinforcement which Cavendish brought to the nascent reformation of chemistry; equally worthy of observation were the means employed to obtain them. The experiment to be made was the combustion of hydrogen with common air; but the object was to compare the weight of the fluid produced in the combustion, with the weight, not of the mixed gases, but of the combining portion of them,—of the hydrogen that is, and the proportion of oxygen which the common air contained. Now this latter was then an unknown quantity. Hence those analyses which he made in 1781, of the atmosphere under all circumstances, at different times of the day, in town and country, in summer and winter, by which he determined its composition more accurately than any of his contemporaries, and with a precision which has scarcely since been exceeded: thus with a knowledge of the specific gravities he was enabled to compute the proportion of oxygen in a given weight of common air, and comparing the weight of the gases with that of the fluid, he found them to correspond. But this experiment had a weak side, in the practical difficulty of collecting the fluid: he next therefore devised a more certain method of examining the question by volume instead of weight, by ascertaining whether the production of the fluid was accompanied by a total disappearance of the combining gases: to a given bulk of atmospheric air, he added, in successive experiments, a gradually decreasing volume of hydrogen gas, and found a point at which the computed volume of oxygen entirely disappeared. But there was yet a possibility of error: the fluid produced might contain something besides water: he analysed it, and found that the water was pure. Not yet satisfied, he repeated the experiment in a simpler form, by burning the hydrogen with oxygen, in place of common air; and here a difference little to have been expected appeared, for, on analysing the fluid he found it to contain not water only, but nitric acid; he traced the acid to its source in the small portion of atmospheric air with which the gases chanced to be contaminated, and inferred that the oxygen and nitrogen which it contains, unite under certain circumstances to form nitric acid. Thus he discovered the cause of its diminution when traversed by the electric spark, and from the residuary defect of the experiment completed the solution of the problem.

This experiment Priestley repeated, and since it is this repetition which M. Arago has mistaken for the first proof of the composition of water, listen, Gentlemen, to his own preface to the account he gives of it;—"Still hearing," he says, "of many objections to the conversion of water into air, I now gave particular attention to an experiment of Mr. Cavendish's concerning the re-conversion of air into water, by decomposing it in conjunction with inflammable air." He then relates the precautions he took in repeating this experiment, expresses his wish that he had a nicer balance, and tells how he collected the fluid by wiping the inside of the glass with filtering paper; but it does not appear, either from his own statement or the still more particular one furnished by Mr. Watt five days afterwards, that he examined the nature of the fluid. Experiments thus conducted could not and did not lead to any solid conclusion; seven years afterwards, we find Priestley expressing himself thus,—"I must say, as I did when I was myself a believer in the decomposition of water, that *I* have never been able to find the full weight of the air in the water produced by the decomposition." And again—"Having never failed, when the experiments were conducted with due attention, to procure some acid whenever I decomposed dephlogisticated and inflammable air in close vessels, I concluded that an acid was the *necessary* result of the union of these two kinds of air, and not water only." Compare these statements, Gentlemen, which have stood on public record for half a century, with those of M. Arago, affirmative that Priestley was the first who *found the full weight of the airs in the water produced by the decomposition*, and claiming for Watt to have been the first person who understood the *re-conversion of air into water*, and ask yourselves, where is the *corpus delicti* to sustain a charge against Cavendish, Blagden, and the printers of the Royal Society's Transactions, of conspiring to defraud men of discoveries which they denied, and notions which they did not even maintain?

In doing this justice to an injured name, I have been led to speak of one whose numerous discoveries attracted in those days the eyes of all Europe to Birmingham, and who deserves to be admired not more for his inventive fertility and indefatigable industry in experiment, than for the honest candour with which he related every fortuitous success and extraneous hint, and the liberal profusion with which he scattered his gold abroad for public use, as fast as he drew it from the mine. It has been one of the charges, Gentlemen, against this Association, that an analysis of the character of Priestley formed a part of its early transactions; that character, drawn by a hand no less judicious than skilful, regarded science alone, and contained not a single particle of political or polemical alloy: if it had, being in the chair when it was read, I should have felt it to be my duty to interfere. Much more would I myself avoid the touching from this chair on any topic which should have a tendency to excite feelings alien to *our* pursuits, and destructive to all social union: but whilst I can well bear to hear our meetings upbraided with such faults as these, there is one point of attack on which I think I ought not to be silent, even though it stands close on the boundaries of those subjects which I would most rigidly exclude. On my own judgment alone I should scarcely venture to meddle with so arduous a question, did I not see those around me who desire it at my hands, as required by the position in which the Association stands.

A century and a half ago the Royal Society met with opponents similar to those whom the Association has to encounter now:—"Their enemies," says Dr. Samuel Johnson, "were for some time very numerous and very acrimonious, for what reason it is hard to conceive, since the philosophers professed not to advance doctrines, but to produce facts, and the most zealous enemy of innovation must admit the gradual progress of experience, however he may oppose hypothetical temerity." They were assailed, Gentlemen, with jokes as well as libels; but there is reason even in ridicule; and, on this subject, the irony of Butler himself is forgotten; but there was also a graver class of men in those days, who saw in the establishment of the Royal Society injury to religion; their names and publications have perished, but the memorial of their apprehensions is embalmed by a writer* whose early history of the Society has been described by his great biographer as "one of the few books which selection of sentiment and elegance of diction have been able to preserve, though written upon a subject flux and transitory."—"I will now proceed," said the episcopal historian, "to the weightiest and most solemn part of my whole undertaking,—to make a defence of the Royal Society and this new experimental learning in respect of the Christian faith; and I am not ignorant in what a slippery place I now stand, and what a tender matter I am entered upon; I know it is almost impossible, without offence, to speak of things of this nature, in which all mankind, each country, and now almost every family, disagree. I cannot expect that what I shall say will escape misrepresentation, though it be said with the greatest simplicity, while I behold that most men do rather value themselves and others on the little differences of religion than on the main substance itself." He then thinks it necessary to employ thirty-three pages in defending the inductive philosophy against the charge of impiety, and concludes with this caution,—"that, above all, men do not strive to make their own opinions adored, while they only seem zealous for the honour of God."

These are bygone days, and Time, gentlemen, which seems to have little effect in removing prejudice, makes great changes at least in circumstances: the philosophy thus early dreaded, has since extended itself on every side; science pervades our manufactures, and science is penetrating to our agriculture; the very amusements, as well as the conveniences, of life, have taken a scientific colour. In these altered circumstances, were any now rash enough, to kindle the dying embers of this obsolete bigotry—to stir up a worse than civil war between the feelings of piety and the deductions of reason, to go forth with the "*argumentum ad odium*" for their only weapon, against a host of facts patiently ascertained, and inferences fairly drawn;—were they to call in the Scriptures to supply their defects, and fasten on them their own crude and ignorant speculations—were they to be seen shifting their ground from one false position to another, all equally untenable, and all assuming to be the sole defenses of the true faith, —what would be the natural consequence of a warfare at once so offensive and so hopeless? what the effect of so many baffled aggressions and self-inflicted defeats? what the fruit which the tree of knowledge would bear, thus injured, in the name of religion, by men who should remove the boundary marks of faith and philosophy, and confound things human and divine?

There are, indeed, certain common points in which reason and revelation mutually illustrate each other; but in order that they may ever be capable of doing so, let us keep their *paths* distinct, and observe their *accordances* alone; otherwise our reasonings run round in a circle, while we endeavour to accommodate physical truth to Scripture, and Scripture to physical truth.

The observation of the true points of accordance in such lines, is one of the most instructive of all studies; and when combined with an honest observation of the discordances also, leads to important conclusions.

There are many branches of inductive inquiry through which these parallel lines may be drawn, and their accordances observed. Thus Sir Isaac Newton has deduced from the history of inventions, the spread of nations, and the present amount of population, that the time for which mankind have existed cannot materially differ from that usually assigned from Scripture. Geology, with less distinctness, points towards the same conclusion. But there are lines of accordance and discordance within the Scriptures themselves. Now, in drawing these lines for human chronology, we find discordances between different versions of almost equal authority, and *that* to such an amount, that while the Hebrew gives 1948 years for the epoch from the Creation to Abraham, the Greek assigns for the same period 3334; and this difference of 1400 years lies not in a single sum, but is divided among successive generations. For the first seven centuries, the larger computation was exclusively followed; for the last four, the whole Western Church has adopted the less. Do these discordances undermine the authority of Scripture? Do they shake—has any one imagined them to shake—the substantial credit of its history of mankind? Yet it is plain that, in its present condition, Scripture does not teach with certainty and exactness the computation of time. The book of Genesis is not then a book of chronology: it is a book which, by a series of genealogies, traces back the various races of men to one common source. Now, in this point, all the versions, and both the volumes of Scripture, concur—to this point all the lines of scientific inquiry converge—the analysis of language, the most legitimate conclusions of physiology and natural history, coincide in the fact that the nations of the earth are of one blood. There is nothing vague or doubtful in this. Reason and religion are here in perfect accord.

Let us proceed from the history of mankind to the general philosophy of nature. No one, I think, can doubt that those who condemned the Copernican system were justified in conceiving that the Scriptures speak of the earth as fixed, and the sun as the moving body. Every one will allow also that this language is ill adapted to the scientific truths of astronomy. We see the folly of any attempt, on this point, to interpret the laws of nature by the expressions of Scripture; and what is the ground of our judgment? We are not all competent to judge between the theory of Copernicus and those which preceded it; but we determine against the seeming evidence of our senses, and against the letter of Scripture, because we know that competent persons have examined and decided the physical question. Now, gentlemen, in Geology we are arrived at the self same point; that is to say, a vast body of the best informed naturalists have examined, by all the various lights of science, and by undeniable methods of investigation

* Spratt, Bishop of Rochester.

tion, the structure of the earth; and however they may differ on less certain points, they all agree in this—that the earth exhibits a succession of stratification, and a series of embedded fossils, which cannot be supposed to have been so stratified, and so embedded, in six days, in a year, or in two thousand years, without supposing also such numerous, such confused, and promiscuous violations of the laws and analogies of the universe, as would confound, not the science of geology alone, but all the principles of natural theology. Here, then, is another point of discordance; and in this case the discordance lies between the language of Scripture and the truths of science.

To understand how this may be explained, let us compare the account of creation given in Genesis with that contained in a composition as old, or older, than this oldest of books,—a composition which, carrying us back some four thousand years into the midst of the patriarchal ages, yet breathes a spirit of no vulgar philosophy; and when it speaks of Him "who hangeth the earth upon nothing," who "maketh a weight for the winds, and weigheth the waters by measure," might tempt us to seek here, with Hutchinson, for the true system of the universe. In that book, I say, we have the first account of the creation of the world, proceeding, as it were, from the mouth of the Creator himself. "The Lord answered Job out of the whirlwind, and said, Where wast thou when I laid the foundations of the earth? declare if thou hast understanding. Who hath laid the measures thereof, or who hath stretched the line upon it, whereupon are the foundations fastened, or who laid the corner-stone, when the morning stars sang together, and all the sons of God shouted for joy?" "Or who shut up the sea with doors, when it brake forth as if it had issued from the womb, when I made the cloud the garment thereof, and thick darkness its swaddling band, and brake up for it my decreed place, and set bars and doors, and said hitherto shalt thou come, and no further, and here shall thy proud waves be staid."

Take, then, these "thoughts that breathe and words that burn," and compress them if you can into some true or some fanciful system of science; teach us where to find "the house wherein darkness dwelleth," to "bind the sweet influence of the Pleiades, and loose the bands of Orion"; explain to us, with respect to one of God's creatures, what the natural process is by which he "drinketh up a river and hasteneth not; and of another, how his breath kindleth coals, and a flame goeth out of his mouth," and then take credit to yourself for vindicating the truth of Scripture; and when you have thus illustrated a composition, by the side of which, till you touched it, the images of Homer and Pindar seem but as prose, go on—instruct us how to interpret that other most ancient book, recorded by the very same hand—take that passage of it which drew forth the admiration of heathen antiquity—borrow for your purpose the deepest thoughts of modern science—substitute, "let there be *ether*, and there was *ether*," for "Let there be light, and there was light." Why does this altered expression fall so flat upon the ear? it is not like the flood of sound which some of you may here have heard responding to these words—it is not like the words themselves, which pour upon the mind at once all the beautiful irradiation and delightful perceptions of light; and yet, Gentlemen, after all, you have not even thus, perhaps, presented a pure scientific view of the act of creation; for when you have conceived this empyreal ether, this boundless and all pervading substance, which vibrates knowledge to our wondering ken out of the unfathomable depths of space, are you prepared to take here your stand? may you not yet find other and still finer links beyond? and if you should, must not the very form of expression, the instantaneous connexion of the thing made with its Maker, "He spake, and it was created," become, as little scientific, as it is, to the understanding of all men, superlatively impressive and sublime?

You see, Gentlemen, what my meaning is. Had it pleased God to endow his servants with supernatural knowledge of the mysteries of nature, they could not have used that knowledge to any practical purpose; they could not have used it so as to carry the truths they were commissioned to preserve into the hearts and imaginations of mankind; and so entirely sensible were they themselves of this, that on subjects thus passing the power of language, they not only meddled not with any system of science, but passed over popular ideas to those highly figurative expressions, which are best adapted to impress transcendental truth.

Who, then, would expect to find in Genesis the chronology or sequence of Creation? who can think that he upholds the authority of Scripture by literal constructions of *such a history*, by concluding from them that the earth was clothed with trees and flowers before the sun was created, or that the great work was measured by six rotations of the earth upon her axis! It scarcely needed the evidence of physical or geological science to teach us that such a mode of interpreting the sacred writings is utterly unsound: when the same author speaks of man as created in the image of God, every one perceives that this is one of the boldest figures which language can produce; and in what but a figurative light can we view the days of *Creation?* what *can* they mean, but that the six grand classes of natural phenomena were, all and each, distinct acts of Divine power, and proceeded from the fiat of a single Creator?

Here, Gentlemen, is a second instance of one of those great points of accordance, where all the conclusions of human science coincide with revealed religion, and none more remarkably than that which has been so falsely termed irreligious Geology; for as Astronomy shows the unity of the Creator through the immensity of space, so does Geology, along the track of unnumbered ages, and through the successive births of beings—still finding in all the uniform design of the same Almighty power, and the varied fruits of the same unexhausted goodness.

Thus, Gentlemen, we have seen in this comparison of two collateral lines of knowledge, certain points of accordance, and certain points of disagreement; we have seen that Scripture furnishes us with no perfect chronology of history, and with no chronology of creation except the creation of man; but we find, also, that it does provide for us, and has evidently aimed at providing for us, from the earliest times to the present hour, the knowledge of two facts:—that all men are the children of one human father, and the handiwork of one Almighty God. Here the coincidence is perfect of every line within and without; here the philosophy of Job and Moses, of every prophet and evangelist, agree; here all the inductions of every branch of science mark the same corresponding points.

And what, Gentlemen, is the common quality of these two facts? Are they not the very facts on which the system of human duty subsists, on which humanity and piety depend?

These truths, Gentlemen, nursed for a thousand years in the ancient Scriptures of the Jews, led forth into new day, and with new accessions of the same kind of knowledge by our holy religion, have walked through the world, and been believed alike by the ignorant and the wise, before our sciences were born: and here observe the methods and the course of Providence;—how as in process of years the current of traditionary belief runs weaker,—how, as the advance of human intellect looks for other kinds of proof, the arts and sciences come in to support these essential truths: printing gives them stability and extension, optics and astronomy pour in an infinity of evidence; comparative anatomy brings up its convictions, and geology subdues the sceptical mind with hitherto unimagined demonstrations.

And now, Gentlemen, I think we are in a condition to draw an inductive conclusion, and even to hazard a prediction. We may safely predict, that truths thus firmly established by evidence, will never be shaken by the researches of that reason which has hitherto lent them all its support; we may clearly point what is that sacred ground on which no unhallowed hypothesis should tread; we are entitled, by the rules of our art, to say to the misnomered philosopher who rashly invades this territory,—These are settled points; settled by every conclusion of the intellect, as well as by every intuition of the heart: stand aloof! disgrace not the name of Science by throwing stones at the Temple of Truth. But for this assembled body of real workmen, amidst their labours of intellectual industry,—the quarrier of the stone, and the fine carvers thereof; the miner that digs the ore, and the smith that fashions it in the fire—for all who are employed on this sacred building, we are justly entitled to claim, that they shall not be forced, like the builders of another temple, to work with arms in their hands—we are bound to wish them God speed: it is our duty, our pride, and pleasure, each in our degree, to aid their efforts, and animate their zeal. Go on, and prosper, Gentlemen, amid the best wishes of the wise and good; look well on the beauty of the fabric you are adorning, and mark its substantial utility; see piety kneeling at its altar, and human infirmities crowding to its gate. With such thoughts within, and such sympathies without, strengthen and regale yourselves amidst your toils, and remember that they carry with them a far higher reward than any human sympathy, in the approbation and the blessing of the great Father of Truth.

TUESDAY, AUGUST 27.

Section A.—MATHEMATICAL AND PHYSICAL SCIENCE.

Mr. Scott Russell brought up the Report of the Committee (Sir John Robison and Mr. Russell) on Waves.—Since the last meeting (see *Athen.* p. 605, 1838), the Committee had continued their researches, and had, in each department, confirmed or corrected the results formerly obtained by them, and had also extended their acquaintance with several interesting phenomena. The first object of their attention, was the determination of the nature and laws of certain kinds of waves. Of these, the most important species was that called by Mr. Russell the Great Solitary Wave, or the Primary Wave of Translation; the second, was the Oscillatory Wave, or secondary species. The recent researches, while they had confirmed and extended to a larger scale the observations of preceding years, have in no respect altered the views formerly stated by this Committee. The form of the wave is that to which the name Hemicycloid has been given; its velocity is that due to half the depth of the fluid, reckoned from the top of the wave to the centre of gravity of the section, where the depth of the channel is not uniform. The motion of the particles, is a motion of permanent translation in the direction of the motion of the wave, through a space equal to double the wave's height; the particles of the water perfectly at rest before the approach of the wave, were lifted up, translated forwards, and were deposited perfectly at rest in their new locations—the translation taking place equally throughout the whole depth of the fluid. It had been stated by Mr. Russell, in a previous Report, that all the data requisite to derive the phenomena of the great wave of translation had been now obtained, and that he himself had found no difficulty in obtaining the laws of this wave from the equations of M. Laplace, on the hypothesis, that the motion of the water particles was not infinitely small, and the oscillations infinitely small, as had been hitherto supposed, but that they had the magnitude and nature actually found in these experiments. During the past year, considerable progress has been made in this examination, and highly satisfactory confirmations of the truth of these views had been obtained, by the labours of Prof. Kelland. Prof. Kelland had also adopted the method of introducing the hypothesis of the particles of the water having the motions observed in the experiments—viz. a motion of permanent translation in a given course; and although his results did not perfectly accord with the experiments, they had presented a much closer approximation to them than previous investigations, and were to be regarded as additions to the theory of the motion of waves. The second subject of inquiry by this Committee regarded the connexion which existed between the motion of waves, and the resistance of fluids to the motion of floating bodies—a subject closely connected with practical navigation. In the continuation of this inquiry, the Committee had received many confirmations of their former views, while they had been extended beyond their former means of observation. The phenomenon of vessels at high velocities riding the wave, had been exhibited to an extent never before witnessed. It had been formerly observed, that when a vessel was, by any means, drawn along the surface of water with a high velocity, it produced a wave, which was a wave of translation, moving with the velocity due to the depth; and that whenever the velocity of the vessel becomes greater than the velo-

city of the wave, the vessel is carried along on the top of the wave with diminished resistance. This phenomenon had been last year exhibited on a scale much more striking than formerly. The wave, divided in two by the prow of the vessel, had risen on each side to a height far greater than that even of the vessel itself; it expanded on each side of the vessel, from stem to stern, in a broad unbroken sheet of water, bearing along the vessel between what seemed to be a pair of extended gossamer wings, giving, at extremely high velocities, a resistance very much less than had hitherto been observed. The third point of inquiry concerns both the theory of hydrodynamics, and the art of practical navigation. What is the form of a solid of least resistance, is a question which has been much discussed since the days of Sir Isaac Newton. What is the shape to be given to a vessel or ship, so that it may be driven by sails, or propelled by oars or steam, with the greatest velocity, or at the least expenditure of power? is the same question, asked by the practical ship-builder, in relation to his art. It had been inferred by Mr. Russell, from certain theoretical considerations, that a certain form, which he called the Wave Form of vessel, would be the form of least resistance, and the form of greatest velocity. This form was, that the lines of anterior displacement should correspond to the outline of the great wave of translation anteriorly, and to the outline of the posterior wave of replacement, towards the stern. This form had been the subject of experiment from 1834 to the present time; and the experiments of last year confirmed the truth of the original supposition, that this form was that of least resistance. A very curious and gratifying confirmation of the truth of the hypothesis had been received during the course of last year. A clergyman in the north of Scotland—the Rev. Mr. Brodie—had seen an account of these experiments in the Reports of the Association, and had deduced from theory, without a knowledge of Mr. Russell's process, results almost precisely the same, so as to give them the most gratifying confirmation, although derived from grounds somewhat different. The views formerly submitted to the Association, on the form of vessel best suited to the practical purposes of navigation, had, during the past year, received very remarkable practical confirmation. A large steam-vessel had been built on the wave principle,—a vessel of 660 tons, with an engine of 220 horse power,—and it had turned out that this was the fastest vessel now in Great Britain. This vessel, built as a pleasure-yacht, combined the qualities of sailing fast, and carrying a large cargo: she was named the *Fire King*, and belonged to Mr. Ashton Smith, of Wales. The last point of inquiry in which this Committee had been engaged during the past year, was the question of the nature and laws of the tidal wave, as propagated along our shores, and up the estuaries of our great rivers. But the nature of its propagation along our shores, after it ceased to be affected by the celestial influence, formed a terrestrial mechanism, with which we were still very imperfectly acquainted. To this subject, the Committee had devoted their attention, and they had made simultaneous observations for this purpose, at thirteen different stations along the Frith of Forth. The Board of Northern Lights, and Mr. Stevenson, their engineer, had afforded every facility for the observations, and results of a very remarkable nature were obtained. It was found, that there were four tides a day in the Forth, instead of two—four high waters and four low waters. Mr. Russell exhibited drawings of these tidal waves, and gave, what he conceived the explanation of a phenomenon, which is, he thinks, much more common than hitherto supposed. It is well known, that the tidal wave which brings high water from the Atlantic to the south-western shores of Great Britain, becomes divided into two parts, one of which passes upwards through the English Channel, and the other passing round the west and north of Ireland and Great Britain, brings high water to the east coast of Scotland and to the Frith of Forth. Now, it appears not to have been recollected, that the other wave, after coming up through the English Channel, and bringing along with the former, high water to London, must pass on northwards, and in doing so, will enter the Frith of Forth considerably earlier than the northern wave passing southwards. Now, this southern wave, smaller, but earlier than the other, appears to enter the Frith, and may be traced at every station. It is followed up, however, very rapidly, by the great northern wave, and the former moving more slowly than the other, according to the law of the great wave of translation, is overtaken by it at the higher parts of the Frith, and being both greatly exaggerated by the form of that channel, produce the two tides of the Frith of Forth. Mr. Russell expressed his opinion, that the tides in the upper part of the Frith of Forth would be found to rise as high above the mean level of the sea, as the tides of the Bristol Channel. The observations on this subject were not, however, completed, but would be finished in the course of next year.

Mr. Byrne asked what was the exact form of the curve which Mr. Russell considered to bound the solid of least resistance? or what was the mathematical formula (or equation) of the curve?—Mr. Russell replied, that it was stated at length in the published reports of the Association.—Prof. Forbes asked, whether the double culmination of the wave might not arise rather from the circumstance of the two waves not coming simultaneously to the mouth of the Firth than to the separation of the waves, to which Mr. Russell had ascribed it? In fact, the waves, as he considered, must be dissimilar in period, or one could not overtop the other; after they had blended into one, the effect would be either to cause the ordinate of the joint wave to be the sum or difference of the ordinates of the waves or waved depression, accordingly as the two waves or waved depression coincided.—Prof. Whewell said, No; they will separate.—Mr. Russell observed, that the waves were found frequently to separate, and the instances he had cited were remarkable exemplifications of this.—Prof. Stevelly believed the difficulty Prof. Forbes laboured under, arose from his not having been present at the Newcastle meeting, when Mr. Russell gave a very minute account of the structure of the wave of translation, which differed from a common undulatory wave in this, that every particle of the fluid had not only a rising and falling motion, but also a motion of translation, sometimes forwards, sometimes backwards. Now, the mere undulatory motion would exactly produce the effects Prof. Forbes expected, and had so well explained; but the motion of translation of the particles would, from its very nature, cause the wave to separate, even after blending into one, as explained by Mr. Russell.—Lieut. Morrison asked whether Mr. Russell had investigated the effect of the descending current of the river, particularly in the time of freshes, on the tide waves? It had been well ascertained by observations on the Thames, that at such times the descending force of the current exercised a very decided influence on the tides; and he could testify the same from his own experience in other places.—Mr. Russell said, the Committee had found that effect very decided, insomuch that in the time of freshes in the Firth, it occasionally obliterated the tide wave.

Prof. Whewell communicated some tide observations, forwarded to him by the Russian Admiral Lütke. These observations supplied—first, the tide hours of various places on the coasts of Lapland, the White Sea, and the Frozen Sea, and the coasts of Nova Zemlia. These observations enable us to follow the progress of the tide mean further than had hitherto been done. Mr. Whewell's map of Cotidal Lines, (the second approximation contained in the Phil. Trans. 1836,) follows the tide only as far as the North Cape of Norway, eastward of Nova Zemlia. Prof. Whewell stated, that he was informed by Admiral Lütke, that in the Frozen Sea, east of Nova Zemlia, there is little or no perceptible tide. The observations communicated by Admiral Lütke, offered various other results, and especially the existence of the *diurnal inequality* in the seas explored by Russian navigators, as on the coast of Kamscatcha, and the west coast of North America.

Prof. Whewell made some observations on Capt. Fitzroy's views of the tides. In the account of the voyage of H.M.S. *Adventure* and *Beagle*, just published, there is an article in the Appendix, containing remarks on the tides. Captain Fitzroy observes, that facts had led him to doubt several of the assertions made in Mr. Whewell's memoir, published in the Philosophical Transactions, 1833, and entitled 'Essay towards a First Approximation to a Map of Cotidal Lines.'* —(Appendix, p. 279.) Prof. Whewell stated, that he conceived that *doubts*, such as Capt. Fitzroy's, are reasonable, till the assertions are fully substantiated by facts. Capt. Fitzroy has further offered an hypothesis of the nature of the tidal motion of the waters of wide oceans, different from the hypothesis of a progressive wave, which is the basis of Prof. Whewell's researches. Capt. Fitzroy conceives, that in the Atlantic and the Pacific, the waters oscillate laterally between the eastern and western shores of these oceans, and thus produce the tides. This supposition would explain such facts as these, that the tide takes place along the whole west coast of South America at the same time; and the supposition might be so modified as to account for the absence of tides in the central part of the ocean. Prof. Whewell stated, that he was not at all disposed to deny, that such a mode of oscillation of the waters of the ocean is possible. Whether such a motion be consistent with the forces exerted by the sun and moon, is a problem of hydrodynamics hitherto unsolved, and probably very difficult. No demonstrative reason, however, has yet been published, to show, that such a motion of the ocean waters may not approach more nearly to their actual motion, than the equilibrium theory, as usually applied, does. When the actual phenomena of the tides of the Atlantic and Pacific have been fully explored, if it appear that they are of the kind supposed by Capt. Fitzroy, it will be very necessary to call upon mathematicians to attempt the solution of the hydrodynamical problem, either in a rigorous or in an approximate shape.

Report on the application of the sum assigned for Tide Calculations, to Prof. Whewell, in a letter from T. G. Bunt, Esq. Bristol.

Bristol, August, 1839.

Dear Sir,—I send you eleven new tide sheets, No. 33 to 43, containing discussions of my new tide-gauge observations, made between September 1837 and June 1839; together with two of the former sheets, No. 25 and 29, showing the correction-curves of lunar parallax and declination, from the Dock Observations of 1836 and 1837. I have also added several new correction-curves to those of *lunar* parallax and declination merely, on sheet No. 29, arising out of my investigations of the solar effects on the times of H.W., and my numerical calculations, undertaken for the purpose of reconciling the discrepancy between the declination curves, numerically and graphically obtained. My suspicions fell on the latter, as I have already informed you;—but after a long and laborious scrutiny, I found that the others were most in fault. All the lunar correction-curves hitherto obtained, were, however, defective, for want of applying the solar correction, the introduction of which has considerably altered, and, I hope, improved them.

From the unexplained residues of time and of height, contained in the sheets for 1834, 5, 6, and 7, (44 Ho. Ant. Ep.) I obtained the corrections for solar effect, first, in a series of 24 curves, for half calendar months, and afterwards for $\left\{\begin{matrix}10^{\circ}\\22^{\circ}\end{matrix}\right\}$ declination, and $\left\{\begin{matrix}8''.45\\8''.70\end{matrix}\right\}$ parallax. The solar correction was also found by means of the numerical calculations of the times of H.W. for 1834, 5, 6, 7, and 1838, which was calculated twice. The results may be seen in sheet No. 29. Another arrangement was made from the residues of time on the sheets for 1834, 5, and 6, for the declination corrections of 5°, 13°, 20°, and 25°. The results, which are laid down on sheet No. 29, strongly favour the supposition that the effects of declination are as their squares. My latest declination curves, on sheet No. 37, for 8°, 19°, and 26°, are still more decisive: the sum of the 12 ordinates between the lines of 8° and 19°, being to those between 19° and 26°, as 57 : 58 by measure on the sheet; and as 57 : 60 by the law of the squares, differing only 1-30th from the law.

In hope of throwing some light on the question of the best anterior epoch of *declination*, considered separately from parallax, I arranged the numerical errors or residue of calculated times for 1837 and 1838, keeping the increasing and decreasing declinations separate, but not distinguishing North declinations from South. I was not, however, able, in this way, to perceive how any change in the declination epoch would be attended with advantage.

An arrangement of the numerical errors in the calculated times for 1837, into two parcels for each month, those following a north transit in one parcel, and those

* Among the points which I could not establish in my own mind, by appeal to facts, were:—"The tides of the Atlantic are, at least in their main features, of a derivative kind, and are propagated from south to north;" "that the tide wave travels from the Cape of Good Hope to the bottom of the Gulph of Guinea, in something less than four hours; that the tide wave travels along this coast (American) from north to south, employing about twelve hours in its motion, from Acapulco to the Strait of Magalhaens;" "from the comparative narrowness of the passage, to the north (of Australia), it is almost certain, that these tides must come from the southern end of the continent." "The derivative tide, which enters great oceans, (North and South Pacific,) from the south-east, is diffused over so wide a space that its amount is greatly reduced."

following a south transit in the other, gave the following results:—

1837. Month.	Mean Error of calculated time of H.W. following a *North* Transit.	Mean Error of calculated time of H.W. following a *South* Transit.
	Dock Observations.	*Dock Observations.*
January.....	3.7 min. too early.	3.7 min. too late.
February	2.4 ditto.	2.4 ditto.
March.....	1.6 ditto.	1.6 ditto.
April	2.3 ditto.	2.3 ditto.
May	2.0 ditto.	2.0 ditto,
June	1.9 ditto.	1.9 ditto.
July	1.4 ditto.	1.4 ditto.
August	3.0 ditto.	3.0 ditto.
September....	2.3 ditto.	2.3 ditto.
October	2.5 ditto.	2.5 ditto.
November....	1.6 ditto.	1.6 ditto.
December	2.3 ditto.	2.3 ditto.

The average of the year gives the observed time of the H.W. following a *north* transit, 2.2 min. *later* than the mean or calculated time; and of the H.W. following a *south* transit, 2.2 min. *earlier* than the mean.

It is quite certain that the diurnal inequality in the intervals at Bristol, is such as to make the time of a H.W. next after a north transit, in the main, *later* than that of a H.W. next after a south transit. This is evident upon the slightest inspection of sheets 37—43, tide-gauge observations.

The effect of this *constant* of diurnal inequality, is positive when the moon's declination (4 days ant.) is north; and negative, when ditto is south; or, in other words, the diurnal inequality in the interval is greatest when the moon has [had] north declination, and least when south. Is it possible to refer this to anything but the moon's difference of distance from the two opposite surfaces of the ocean?

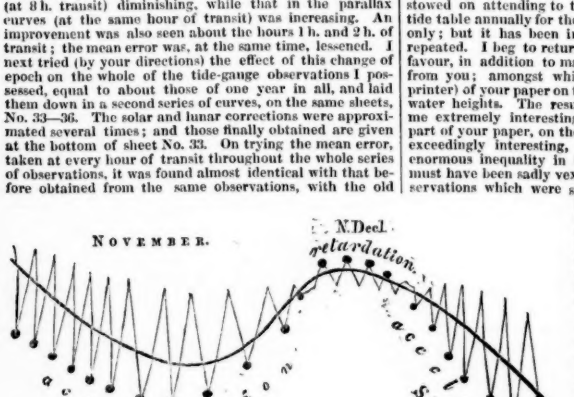
I next proceeded to discuss the observations of the times of H.W. from my tide-gauge register, using the mean between two equal altitudes, taken at one-fourth of the distance from H.W. to mean water, instead of the actual or observed time of H.W. In the first discussion, of about twelve months' observations, in this way, made between Sept. 1837 and Jan. 1839, and laid down on sheets 33—37, the anterior epoch of 44 hours was employed, as formerly. A comparison of the lunar correction-curves from the different epochs of 32 h., 45 h., and 56 h., seemed to indicate that the epoch of 38 h., intermediate between those of 32 h. and 44 h., would probably afford correction curves of parallax and declination approximating more closely to each other, both in form and in magnitude. At your request, I made the trial, first, with about six months' observations, which, being treated in the usual manner, yielded, after several approximations, and a new solar correction, curves of lunar declination and parallax, of the shape that had been anticipated; the second loop in the declination curves (at 8 h. transit) diminishing, while that in the parallax curves (at the same hour of transit) was increasing. An improvement was also seen about the hours 1 h. and 2 h. of transit; the mean error was, at the same time, lessened. I next tried (by your directions) the effect of this change of epoch on the whole of the tide-gauge observations I possessed, equal to about those of one year in all, and laid them down in a second series of curves, on the same sheets, No. 33—36. The solar and lunar corrections were approximated several times; and those finally obtained are given at the bottom of sheet No. 33. On trying the mean error, taken at every hour of transit throughout the whole series of observations, it was found almost identical with that before obtained from the same observations, with the old epoch of 44 hours, viz. 2¼ min. very nearly. You then requested me to make a further trial, in order to determine, if possible, whether the new epoch was better than the old one. To do this properly, I found that it would be necessary to draw the curves of observation afresh, and to interpolate the times of transit, for every 6 and 18 hours, between those given in the Nautical Almanack, with greater nicety, namely, with second differences and decimals of minutes. I therefore made the necessary corrections in the intervals, and laid them down anew. You know we had proposed to use a larger number of observations in our second trial of the comparative merits of the two epochs; but as it appeared, upon further consideration, that this would not give a fair comparison, unless the same extension of the observations were also tried with the old epoch, which would have greatly added to the labour, I determined to confine myself to the same observations, while using the epoch of 38 h., as had been employed with the epoch of 44 hours, which was, in fact, to go over the ground a second time with the shorter epoch, endeavouring to avoid any errors I might have incurred before, such as that I have just explained. The corrections I eventually obtained, and laid down on sheet No. 37, are, I believe, for the moon's effects a fourth approximation, and for the sun's a third. On carefully measuring the mean residual error after the two processes, I find a *small* improvement in that of the 38 h. epoch, which is now 2.394 min.; while that of the 44 h. epoch is 2.510 min.

Whatever may be thought of this minute difference, which accidental circumstances may have, in some degree, modified, I think no one can hesitate to give to our new curves of lunar parallax and declination, a preference before those numbered 1 and 1 on sheet 29, which were considered so good only twelve months ago. A discussion of heights would probably afford additional evidence in favour of our new epoch, as there is little doubt that the maximum height would fall much nearer than formerly to the hour of 0 h. transit: though I believe it will now be a little to the *right hand* of that hour line, as though the epoch were rather too much shortened. If similarity of form between the parallax and declination correction-curves be admitted as the proper test of the anterior epoch, it is pretty evident that 38 h. is better than any other that we have.

The observations of displacement of summit of the tide-curves, or differences of equal altitude times and actual times of H.W. are laid down on sheets No. 33—36, and the resulting corrections on No. 33. The observations are continued from January to June, 1839, on sheets No. 42 and 3. I had transferred all these observations to a separate sheet, and begun to discuss them afresh; but seeing no reason for thinking I could throw any further light on the subject just at present, I laid it aside until more observations had been made, or until you favoured me with your opinion as to the best mode of proceeding.

You will be pleased at hearing that Dr. Carpenter's exertions, so ably seconded by your letter, have succeeded in procuring a grant of 50*l.* from the Corporation, in addition to a previous one of 20*l.* from the Society of Merchants, since increased to 25*l.*, making together 75*l.*, which has been paid me in consideration of my time and labour bestowed on attending to the tide gauge, and calculating a tide table annually for the port. These grants are donations only; but it has been intimated to me that they may be repeated. I beg to return you my warmest thanks for this favour, in addition to many others which I have received from you; amongst which is the proof copy (from the printer) of your paper on the Plymouth low water and mean water heights. The results you have obtained appear to me extremely interesting and satisfactory. The second part of your paper, on the Indian tide observations, is also exceedingly interesting, especially what relates to that enormous inequality in the intervals at Bassadore. You must have been sadly vexed at the miserably defective observations which were sent you. I hope you will, by and by, receive some better observations, when the two machines are completed and sent out, which, as you are probably aware, I am superintending, for the East India Company, who have given orders for them to be made just like my own.

I laid down the Bassadore intervals, for the sake of looking at the inequality. I have not the diagram at hand, but I remember being struck with the great disparity between the *acceleration* of the time of H.W., after a given transit—say the superior—when the moon was on *one* side of the equator, and the *retardation* when she was on the *other*; as in the annexed diagram.



Part of this may perhaps be due to the sun, which has there considerable south declination; but if the same thing be observed in the summer months, shall we not have here another case similar to Bristol, where the H.W. next after a *south* transit, is about three times out of four, all through the year, *earlier* than that after a *north* transit, as I have already remarked?

I am, dear sir,
Yours very respectfully,
Thos. G. Bunt.

Mr. Byrne asked whether Prof. Whewell had given up his views or his researches respecting a level line, as dependent on the mean between high and low water. —Prof. Whewell replied, that he had not. A report would be laid before the Section, in the progress of its proceedings on the subject, and a grant had already been recommended by the Committee, for carrying on the inquiry.—Prof. Powell inquired, whether the very remarkable circumstance alluded to by Prof. Whewell, of there being no tide in the middle of the Pacific, near the South Sea Islands, would not find a more probable solution in some, as yet unobserved, interference of tidal waves, propagated from various quarters, rather than the very unlikely one, of an oscillatory motion of the entire mass of the ocean. —Mr. Russell said, he should require stronger evidence than any he had yet seen, before he could be induced to give up the conclusions respecting the propagation of the great tidal wave, to which the researches of Prof. Whewell had led. As to the propagation of a tide wave from the north, southwards along the west coast of South America, he doubted it. With respect to the circumstance of there being no tide in the neighbourhood of the South Sea Islands, he thought it probable that the coral reefs in that part of the ocean would have the effect of obliterating the tide wave, for he found during his researches on waves, that the slightest obstacles placed at the bottom of the trough were sufficient almost instantly to obliterate the wave, insomuch that, whereas at first he had to wait after one experiment sometimes fifteen or twenty minutes, before the surface was still enough to commence the next, after he discovered the fact, he could at pleasure quiet the surface, by elevating certain pieces of wood, which, in general lay even with the bottom. Now, the coral reefs were just such obstacles; and the tide wave being clearly proved a wave of translation, he had little doubt in his own mind, that they would furnish an explanation of the fact.

On the subject of a resolution adopted by the Meeting of the British Association, at Newcastle, to the following effect:—

"That Sir J. Herschel be requested to superintend the reduction of meteorological observations made at the Equinoxes and Solstices, agreeably to his recommendation, and that 100*l.* be placed at his disposal for that purpose,"—

Sir J. Herschel reports,—That he has, within the course of the year elapsed since the last meeting of the Association received several series of observations from distant stations completing wholly or in part the series before transmitted from those stations, but that several are still deficient, which, however, must now be considered either as irrecoverably lost, or as never having been made. That, partly owing to the comparative inutility of reducing incomplete series, and partly to the pressure of other business, especially to that arising from the Magnetic Expedition and observatories, which will form the subject of a distinct report, he has been prevented hitherto from making material progress in preparing the observations in question for reduction, but hopes, before the next meeting, to get them executed; and requests the grant of the Association may be continued for that purpose, and that the surplus, if any, may be made applicable to the reduction of other meteorological observations communicated to him while resident at the Cape, and since, and not made at the solstices and equinoxes, but in the form of monthly registers.

August 14, 1839. J. F. W. Herschel.

On the subject of a resolution adopted by the Meeting of the British Association, held at Newcastle, in August, 1838, to the following effect:—

"Resolved,—That Sir J. Herschel and Mr. Baily be requested to make application to Government for increase in the instrumental power of the Royal Observatory at the Cape of Good Hope, and the addition of at least one assistant to that establishment,"—

The Committee named in this resolution report, —That application had already been made to Government, previous to the date of this resolution, viz., on the 29th of June, 1838, and on other occasions, to the effect therein mentioned, and that in consequence of those applications, aided, no doubt, by a knowledge of its being the wish of the British Association,—such wish having been communicated to Lord Minto by Sir J. Herschel, in a letter, dated November 5, 1838,—they have great pleasure in being able to state:—1st, That ——— Mann, Esq. has been appointed as an additional assistant to the Cape Observatory, at a liberal and sufficient salary, and is already on his voyage thither, to take on himself the duties of his office. 2nd, That Jones's mural circle, hitherto used at Greenwich, has been despatched, by the orders of the Lords Commissioners of the Admiralty, to supply the place of the defective one in use up to that time at the Cape, and is already probably arrived there,—an improvement of essential importance, the Greenwich instrument having been shown, by many years' trial in the hands of Messrs. Pond and Airy, to be of the highest excellence. The liberality of Mr. Airy in resigning this noble instrument to his brother astronomer, cannot, in your Committee's opinion, be too highly estimated. 3rd, That, for the purpose of enabling Mr. Maclear, the Director of the Cape Observatory, to prosecute with effect the remeasurement of Lacaille's arc of the meridian at the Cape, the Lords

Commissioners of the Admiralty have procured, from the Board of Ordnance, the use of Colonel Colby's excellent compensation measuring bars, the same which have been employed by him in the measurement of the Irish base, which are now actually on their voyage to perform a similar office at the Cape. 4th, That the Lords Commissioners of the Admiralty have, in pursuance of the same object, applied for and obtained the use of an excellent theodolite, the property of the Royal Astronomical Society, and by them liberally granted for the purpose of remeasuring Lacaille's triangles.

August 17, 1839. J. F. W. Herschel.

'Notice respecting the Use of Mica in Polarizing Light,' by Professor Forbes.—The author explained the method of preparing mica used by him since 1836, for the polarization of heat and light. The mica is exposed for a short time to an intense heat in an open fire, by which the laminæ are so subdivided, that a pellicle of extreme thinness contains a sufficient number of reflecting surfaces, to polarize very completely the light or heat transmitted through it at a certain degree of obliquity. He next stated, that being struck by the resemblance to metallic lustre which the mica acquires in this process, he had examined (also in 1836,) some of its leading properties with regard to light, and he found, 1st, that the light reflected from a plate of mica so prepared, (which light is very intense,) is but feebly polarized in the plane of incidence; and 2nd, that the reflection so far resembles that at metallic surfaces, that when plane-polarized light is reflected from it, the plane of reflection being inclined to that of primitive polarization, the light is found to be elliptically polarized. The latter fact he stated to be in a great measure explained by a remark made by Professor Lloyd, to whom he had mentioned the experiment yesterday. The observation, and also the theory of it, cannot fail, he thinks, to be important in illustrating the nature of metallic reflection, which is at present so actively discussed.

Prof. Forbes said he would reserve any further observations until after Prof. Lloyd had given to the Section an explanation of the cause of the circular polarization of light.

Prof. Lloyd observed, that, simple as this discovery of Prof. Forbes might appear to a person not conversant with this intricate subject, yet he considered it as highly important, not only as furnishing a simple method of polarizing light elliptically and circularly, more simple than any previously in use, but would also essentially aid the researches of those engaged in perfecting the theory of this interesting branch of physical optics; indeed, did time permit, he thought he could show that it furnished a clue at least to the solution of some of the difficulties which had hitherto opposed the progress of inquiry. The point which Prof. Forbes wished him to explain was this:—in polarizing light elliptically or circularly, it was well known that the condition to be obtained was, that two rays should encounter one another in different phases, or to speak the conventional language, of which one was accelerated by a half or some proportional part of a wave, while the other was either similarly or dissimilarly retarded. Now, in this case, this condition was obtained, and with respect to a greater quantity of rays than in other processes, for those rays which, after being reflected at the first surface of those very thin laminæ of mica which they met, were afterwards encountered by those rays which passed into the laminæ, were reflected at the hinder surface, and came out again after refraction at the first. Now it was obvious that the necessary conditions of acceleration and retardation of the rays could be thus obtained; and the vast multitude of exceedingly thin laminæ which were produced in the plate of mica, by the process of Prof. Forbes, far exceeded anything which, by any process merely mechanical, however delicate, we could hope to obtain. There was another point of view in which he thought that these researches of Prof. Forbes would be of the utmost consequence; he meant, as tending to throw a light upon the internal structure of metals. Young had, with his usual sagacity, long since conjectured, from the well-known fact that very thin leaves of gold transmit greenish light with almost unimpaired regularity, that the surfaces of all metallic plates consisted of very thin laminæ, pervious to light, and that the phenomena of their polarizing influences depended upon this. Fresnel had followed out this conception, and traced mathematically the mode in which the polarization would take place. The present researches not only confirmed these views, which were heretofore only to be deemed as founded on conjecture, but actually showed how, under certain conditions, elliptical and circular polarization could be obtained by a method similar to the method of producing ordinary polarization.—Prof. Powell fully concurred in considering these researches of the utmost importance to the progress of this intricate branch of Physics. He hoped that Prof. Forbes would extend his investigations to all other bodies that were in any manner analogous to the metals. He had himself observed instances where thin films of the oxidated surface of a speculum, so thin as in no respect to interfere with the regularity or even brightness of the image, exercised a very decided polarizing effect upon the reflected light.

'On the Use of the Oxy-Hydrogen Microscope in exhibiting the Phenomena of Polarization,' by J. F. Goddard.

I first (said Mr. Goddard) turned my attention to this subject in the spring of 1834, and in the autumn of 1836 I constructed a polariscope without the use of tourmalines, which enabled me to illustrate this singularly beautiful subject, by exhibiting *all* the different phenomena, for the first time, with the oxy-hydrogen microscope. It is true some few of the rings surrounding the optic axes of double-refracting crystals, were shown, upon a small scale, by Mr. Cary's polariscope, at his Exhibition in Bond Street, in 1834; but this was done by means of a pair of tourmalines arranged in a similar manner to the apparatus described by Sir J. Herschel in his Treatise on Light. These, however, are not microscopic objects, as they cannot be magnified, and are shown by merely placing the tourmalines with the crystal between them at the apex of a cone of rays. All the other phenomena, which are infinitely more numerous and varied, require a very different arrangement, which it is impossible to effect with tourmaline. In the description of my polariscope, published in the Transactions of the Society of Arts, the polarizing mirror will be found to be placed after the third condensing lens, which is only used with the high magnifying powers; this arrangement I was compelled to adopt, as the microscope had been previously made, and would not admit of any other. But I have since had one constructed, in which I could introduce every improvement that my experience suggested,—and one of the most important was to place the polarizing mirror much nearer the light, so that I can now use, with the polariscope, the lowest magnifying powers, and consequently exhibit much larger illustrations and designs in selenite; also the different forms of unannealed glass; and not only greatly to extend the variety of experiments and illustrations, but much to improve the splendour of them all. The most important of these is in the analyzing part of the apparatus. Having obtained an unusually fine plate of tourmaline, I tried various experiments with it and other means, from which it appears much more can be done with a bundle of films of mica, when bleached and properly constructed for analyzing, than can be effected by any tourmalines, however good. Prof. Forbes, in his experiments on the polarization of heat, first employed bundles of bleached mica,—his process of bleaching which, by heat, he has already published; but, for experiments on light, the process must not be carried so far, and requires to be conducted with great care, for, when raised beyond a bright red heat, the mica blisters and becomes unfit for these purposes, being broken into very small portions, that are incapable of transmitting a clear and distinct image, from the number of unequal refractions which the light undergoes in different parts: whilst the heat, if properly regulated, will drive off all the colour of the mica, without its being blistered, it may afterwards be easily divided into sufficiently thin laminæ, that about eighteen of them, placed between two plates of thin glass (to protect them from being scratched), form the best means of analyzing, and by means of which both complementary images may be shown at the same time,—by using two screens, one to receive the refracted image, and the other to receive the reflected image,—thus furnishing the means of exhibiting, with singular effect, all the beautiful phenomena of polarized light.

'Meteorological Observations made at Great Malvern during the Years 1835, 1836, 1837, and 1838,' by Mr. Addison.—In these tables, the mean results for every month, and for the various seasons, and for each year, have been computed. Great Malvern, in Worcestershire, has an elevation of about 500 feet. Great differences of temperature have been observed within short distances—the lower localities being frequently very much colder than the more elevated ones. Thus, on three or four occasions, drops of rain have fallen, with the thermometer in the vale at 24°. The dew point is subject to greater variations, and frequently falls to a much lower point in the higher situations, except when the temperatures are very different; it then appears, that the dew point is frequently very low, in the cold, misty, foggy air of the valley. From these tables, it appears, that the mean temperature of Malvern is 47.7; the highest annual mean is 49.1, in 1835; and the lowest 46.5, in 1838; being a difference of 2.5 between these two years. The mean barometer is 29.386; and the mean dew point, at 9 A.M., 43.7. When the mean temperature of the year is higher, the mean dew point also is higher; thus, in 1835, mean temperature, 49.1—mean dew point, 44.7; in 1838, mean temperature, 46.5—mean dew point, 42.4. In 1837, the lowest temperature of the year occurred in the night of the 25th of March. The maximum of the barometer, in three out of the four above-mentioned years, occurred in the first week of January. The minimum of the barometer, in three out of four years, occurred in November. The range of temperature during the four years, from 9° on the 20th of January 1838, to 84° on the 5th of July 1836, is 75°. The range of the barometer, from 28.010, on the 29th of November 1838, to 30.228, on the 14th of October 1837, is 2.2 inches. The aurora borealis was observed in November, 1835; in May, 1836; in February, March, April, August, October, and November, 1837; and in September, 1838. A remarkable noise was heard at 4 P.M. on the 4th of August, 1835, like a loud clap of thunder, the air at the time being quite free from cloud, and the sun hot and brilliant. Very high winds, with the air at the dew point, occasion a large evaporation.

Prof. Powell asked, whether Mr. Addison had, at any time during the summer, observed a remarkable fall of meteoric stars.—Mr. Addison replied, that on the 15th of November, 1832, having been surprised by observing, through a break in the clouds, something like a shower of stars, he ascended the neighbouring Malvern hills, to a height sufficient to raise him above the clouds, when a spectacle was unfolded to his view such as he never expected again to behold; the number of shooting stars or meteors was so great, that he could compare it to nothing so well as to a shower of fire: each star as it fell seemed to leave a long train of fire after it, for some seconds. He had his seconds watch with him, and was thus able to count forty-eight in a minute flitting about.—Prof. Powell saw at Tunbridge Wells, on the 10th of this present month (August), a very brilliant exhibition of these meteors; they averaged from fifteen to twenty in the quarter of an hour; they all left trains of light after them: the motion of all was from north towards the south.—Lieut. Morrison asked whether he had rightly understood Mr. Addison to say, that he had observed rain to fall at the time that the external thermometer stood so low as 24° Fahr.—Mr. Addison replied, Yes; and further, that in descending a distance of less than a quarter of a mile, he had observed the thermometer fall, on some occasions, no less than 17°.—Prof. Stevelly inquired, whether this occurred in passing into a cloud?—Mr. Addison replied, Yes.—Prof. Stevelly said, that accounted for the fall of the thermometer; and it was an observation of the more value, inasmuch as it was without the ambiguity which results when entering a cloud from a lower stratum. He had himself frequently observed the thermometer to fall many degrees in passing into a cloud; but, in every instance, he had proceeded from a lower station to the cloud. There could be little doubt, that the instances referred to, where the lower regions of the atmosphere were colder than those above, were cases analogous to those in which dew was deposited.—Mr. Baily said, that unquestionably it was so, otherwise it would be a very per-

plexing fact, for the natural undisturbed constitution of the atmosphere was exactly the reverse.—Mr. Julius Jeffreys remarked, that the scantiness of our information respecting the meteorology of very elevated regions, led him to offer a few observations. He had had rather an extensive field of inquiry in the year 1824, when he visited the higher range of mountains in the protected States in the Himalayas, for the purpose of conducting inquiries into the meteorology of those regions, and the character of the climate, in a medical point of view. The space he travelled amounted to near 200 miles. His observations were made during six months, and upon mountain heights, and in their subjacent valleys, from an elevation of 16,300 feet, down to 4,000, and of valleys, from one nearly 14,000 feet high, to 3,000. With regard to the temperature of mountain heights, he found it the case, without an exception, that the diurnal variations were very small, rarely exceeding 12°, and sometimes only 4° or 5°, while the diurnal variations of temperature in the valleys were so great, that commonly the minimum of the night was 30°, and sometimes 40°, and, on one occasion, nearly 48°, below the maximum of the day. Again, he found almost invariably, that the minimum temperature of the subjacent valley, compared with that of the height, was not that due to their relative elevations, which would have made the valley warmer, but the minimum was even lower than that of the height above, while the maximum was much higher in the valley. Thus, although the mean temperature of the valley was higher than that of the height, its minimum was always lower, and the maximum higher, than of the former, where the difference of elevation did not exceed 2,000 feet, or 2,500. In one valley, 6,500 feet above the sea, the thermometer, five feet from the ground, fell to 40° at night, and in the day rose to 88°—a difference of 48°—while on the narrow mountain ridge, 1,500 feet above, it fell only to 50°, and rose little above 60°. This was an extreme case. The movements of the barometer at those heights are frequent, it being affected by the expansion and contraction of the atmospheric column below, from changes in its temperature, whereby a greater or smaller quantity of air exists above the level of the observation. The hygrometric condition of the air was very interesting: its phenomena corroborated certain of Prof. Daniell's views. The medical application of these meteorological inquiries was interesting, and led Mr. Jeffreys to publish an essay, to establish the fact, among other points, that a removal of a considerable portion of the atmospheric pressure from the surface of the human body, must conduce to the restoration of the function of the skin, when exhausted by excess of duty in a tropical climate, and sympathy with a debilitated liver. At that time, no sanatory stations existed in those mountain regions, and a prejudice had been raised against the climate. The Inspector General of His Majesty's forces considered these views to be established, and, in consequence, recommended the formation of the Sanatorium at Landour, which was followed by others.

'On certain Meteorological Phenomena in the Ghâts of Western India,' by Col. Sykes.—In the proceedings of the Physical Section at the meeting at Newcastle, the incidental mention of the annual fall of very many feet of rain in certain localities of India, instead of a few inches, as is the case in Europe, caused, I was told, some surprise, and the expression of a doubt whether the fact had been ascertained with sufficient precision, and by competent persons. I was not present on the occasion alluded to, but the doubt having been brought to my notice subsequently, I lost no time in applying to a friend to procure for me the official meteorological records kept by order of the government of Bombay at the convalescent station of Mahabuleshwar, which records I knew would afford sufficient evidence to remove all doubts, at least so far as related to one locality; and I have now the pleasure of submitting the abstract of the Meteorology for 1834; the observations being made by Dr. Murray, the medical officer in charge of that station.—The station is situated lat. 17° 58′ 53″ N. and long. 73° 29′ 50″ W., near the western scarp of the Ghâts, or mountain chain extending from Surat to Cape Comorin, and varying from 1,000 to 8,000 feet in height. The elevation of the table land at Mahabuleshwar averages 4,500 feet. The temperature of a spring is 65.5 Fahr., and the mean temperature of the air is nearly the same. There is a good deal of forest along the Ghâts, but in belts and patches, so that the wood can have little effect on the phenomena which I am about to describe. In this table land is the source of the celebrated Kistnah river, which runs across the peninsula.

The following table shows the state of the thermometer, fall of rain, &c. at the station.

Months.	Mean Temperature.	Mean Maximum.	Mean Minimum.	Mean Variation.	Fall of Rain in Inches.	Direction and force of the Wind.	
						A.M.	P.M.
January	65.7	71.2	60.2	11.1	—	E. light	E.N.E. & W. light
February ..	67.5	73.7	61.3	12.4	0.25	E.N.E. light	W.N.W. light
March.....	74.0	81.7	66.4	15.3	—	N.E. light	W. & N.W. fresh
April	74.4	82.2	66.7	15.5	Two light showers	N.E. light	W. light
May.....	73.9	80.6	67.3	13.3	0.16	W.N.W. & S.W fresh	W.N.W. & S.W. fresh & strong
June	66.3	69.3	63.4	5.9	32.03	S.W. high & fresh	S.W. high & fresh
July	64.9	66.6	63.2	3.4	118.60	S.W. strong	S.W. strong
August	65.3	66.9	63.8	3.1	75.91	S.W. high	S.W. high
September..	65.0	66.4	63.6	2.8	65.97	S.W. fresh	S.W. fresh
October	65.5	69.4	61.7	7.7	9.29	N.E. fresh	N.E. & S.W. light, E.N.E. fresh
November..	63.5	69.5	57.5	12.0	—	E.N.E. high	E.N.E. fresh
December ..	62.3	68.4	56.2	12.2	One light shower	E.N.E. fresh	E.N.E. & W. light
Mean	67.3	72.1	62.6	9.5	302.21		

Hence it appears that the mean temperature of 1834 was 67.3 Fahr., that of the hottest month (April) 74.4, that of the coldest month (Dec.) 62.3. The mean maximum (April) 82.2, and the mean minimum (Dec.) 56.2. The mean variation was greatest in April, 15.5, and least in September, 2.8; and the mean variation for the year was only 9.5. The fall of rain was prodigious, being equal to 25 feet 2 inches, and this enormous mass of water fell almost entirely in the months of June, July, August and September. General Lodwich, who transmitted to me the official register says, "I sent to you a copy of Dr. Murray's meteorological table. The inches of rain are no less than 302.21. This will astonish the philosophers, but it would do more than astonish them, had they the opportunity of seeing and hearing the rain fall in torrents through a dense fog or mass of clouds which lie upon the ground for perhaps six weeks together, with a temperature by no means cold, and with little variation." The excessive fall of rain along the line of the Ghâts does not seem to be incompatible with health, for the military detachment stationed permanently at Mahabuleshwar is not characterized by any unusual sickness; and statistical returns of the population on the hills are as healthy as those of the table-lands to the eastward. It now remains to notice some striking facts on the western side of the peninsula. The quantity differs exceedingly between the coast, the Ghâts within fifty miles of the coast, and the table-land westward of the Ghâts. The mean at Bombay is 80.69. Dr. Murray's shows a fall in the hills, at the elevation 4,500 feet, of 302 inches, and my records at Poonah give a mean annual fall of 23.43 inches. The solution of the causes of the anomalous fall of rain does not offer any considerable difficulties. The enormous mass of vapour taken up from the Indian Ocean on approaching India, does not appear to have its upper surface at a greater elevation than five or six thousand feet, while the stratum is of great thickness; and I can bear testimony to its lower surface being below fifteen or eighteen hundred feet. The temperature of the air over the equator is necessarily very high, and its capacity for the support of aqueous vapour is proportioned to its temperature. The vapour is converted into rain, as it is driven into air of lower temperature; and, as the temperature gradually lowers proceeding to the north, and approaching the land, it follows, that out at sea, and along shore, with equal supplies of vapour, a less quantity of it would be converted into rain. It may be advantageous to advert to the combined elements of the mean temperature and elevation of Mahabuleshwar and Poonah, as affording data for estimating the value in feet due to a degree of diminution of temperature in ascending above the level of the sea. The mean temperature of Bombay is 82; that of Mahabuleshwar 65.5; and the elevation of the latter, 4,500, giving 272.7 feet to each degree. The mean temperature of Poonah is 77.7; and its elevation above the sea 1,823 feet, giving 424 feet to a degree. The difference of mean temperature between Mahabuleshwar and Poonah is 12.2; and the difference of elevation, 2,677 feet; giving, therefore, 219.4 feet only. I thus add to the discrepancies already before the public on the subject, for Humboldt, in one place, gives 328 feet; in another 364 feet; in a third place, 342 feet, and, in his Personal Narrative, 309 feet; and Dr. Ure gives 200 feet. The calculations between Bombay and Poonah, and the latter place and Mahabuleshwar, indicate accelerated diminution of temperature in similar spaces in the ratio of the elevation; and this will also be subject to modifications contingent on the latitude.

Prof. Stevelly observed, that this paper originated in a conversation, which took place in the Mathematical and Physical Section, at the Dublin meeting, respecting excessive falls of rain and snow; remarks were made on a fact recorded in the report on Meteorology, by Professor Forbes, that in some place among the Swiss Alps, thirty inches of rain had fallen in one day. Several members expressed doubts of the correctness of the register from which this was taken, and of this number he was one; and it was observable, that in the table produced by Col. Sykes it was the month's rain which was recorded, and not any separate day, and the greatest daily average was only eight inches; this, therefore, so far from confirming the record of the Swiss fall of rain, rather tended to discredit it, for it was well known that the rain fell in torrents in India at certain states of the monsoon.—Lieut. Morrison remarked, that it was a curious coincidence, that from the table just produced, the most rainy month in India was July, which was also the month when most rain fell in England.—Mr. Stevelly said, it was to have been anticipated from a general law, for within the tropics the rains commenced just after the sun had passed vertically over the parallel of latitude in his annual course, and in extra-tropical places soon after he had approached nearest to the circle of declination which was vertical to the place.

Section B.—Chemistry and Mineralogy.

'Report of some Galvanic Experiments to determine the existence or non-existence of Electrical Currents among Stratified Rocks, particularly those of the Mountain Limestone formation, constituting the Lead Measures of Alston Moor,' by H. L. Pattinson. The object of this paper was to report the result of experiments made at the request of the Section last year. Mr. Richardson was appointed to act with Mr. Pattinson, but being unable to accompany him at the time required, Mr. Pattinson obtained the assistance of Mr. Leithart, who resided at the scene of the experiments. In the lead district of Alston Moor, the mountain limestone alternates with indurated clay and sandstone (called technically *plate* and *hazel*), with remarkable frequency and regularity. A section of the strata in this part of the country was published in 1800, by W. Miller; but a far more complete and accurate section was published by Mr. Westgarth Foster in 1809, which is also contained in the 45th volume of the *Philosophical Magazine*. The depth of the strata amounts to 259 yards. The author briefly stated the result of the experiments of himself and colleague, as to the occurrence of electrical currents in these strata. Their first experiment was to determine if any difference existed in the electrical condition of the limestone stratum, called the Great Limestone in Foster's

section, and a soft sandstone lying immediately under it. The experiment was performed in a level driven partly in each stratum, at a mine called White Well, near Alston, and the spot selected was at a distance from any vein. They proceeded by fixing, about twelve inches above the under surface of the limestone, in holes bored for the purpose, twelve hollow copper cylinders, each five inches long by one inch in diameter, which were firmly secured in the holes, and forced strongly into contact with the rock by means of wooden plugs driven within the cylinder. With these cylinders a copper wire $\frac{1}{20}$th of an inch in diameter, was connected, and one end of this wire attached to one of the wires of a delicate galvanometer: in the same way twelve copper cylinders were inserted in holes made in the sandstone, about a foot from its upper surface, and to these were attached a similar wire, which was brought in contact with the other end of the galvanometer wire, so that any current between the two strata would be immediately perceived by its action upon the needle. On making the contacts no motion whatever was produced, and every expedient for increasing the effect of a feeble action was resorted to,—such as making contact at short and equal intervals, the interval being the time required for one vibration of the needle, which was previously determined: at the same time, the sensibility of the instrument was such, that a plate of zinc and a plate of copper, each one-eighth of an inch square, in pure water, produced a most distinct action; the galvanic action, if any existed, between this limestone and sandstone stratum where the experiment was performed, was, as collected by the metallic surfaces in contact with each, decidedly less than that of a pair of zinc and copper plates, each one-eighth of an inch square, in pure water. The next experiment was upon a stratum of limestone and a stratum of hazel, having a plate bed between them, and the situation selected was a shaft near Alston, called Water Green's Shaft. The depth of the strata was sixty-one yards. The limestone pitched upon for experiment was the five yards limestone, as it is called, and the sandstone the six fathoms hazel, and the plate bed between which is three feet in thickness. There was a flooring or *bunyan* across the shaft at this plate, and a level driven out of the shaft to a small distance, in which the galvanometer was placed. The situation was at a distance from any vein, and all the circumstances were of the most favourable nature for performing the experiment with the utmost completeness and precision. Six holes, each two feet deep, were bored in the sandstone, a foot above its under surface, and six similar holes were bored in the limestone, a foot below its upper surface: into these holes were introduced twelve slips of sheet copper, each two feet long by two inches wide, bent so as to be semi-cylindrical, and they were fixed and *stemmed* very tightly into the holes, so as to secure the most perfect contact. The six copper slips in the hazel were connected with a copper wire $\frac{1}{20}$th of an inch in diameter, the end of which, as before, was attached to a pole of the galvanometer, and the six coppers in the limestone were similarly connected together and brought in contact with the other pole of the galvanometer. Every possible care was taken in attaching the wires, and in making the contacts; but the needle of the galvanometer did not show the slightest current, although at the same time it was fully sensible to the action of the plates of zinc and copper one-eighth of an inch square, in pure water. When the contacts were made at intervals, so as to be isochronous with the times of vibration of the needle, and continued for several minutes, there was still no appreciable effort whatever. It was thus fully established, that, under the circumstances detailed, there was no current given off equal in amount to that excited by a pair of plates, of zinc and copper, one-eighth of an inch square, in pure water. Indeed, when we reflect upon the thickness and imperfectly conducting nature of the strata, no very obvious current could be expected. It is, however, still possible, that, by using a more delicate galvanometer, bringing into contact with each stratum a larger surface of metal, and including a greater number of alternations of strata within the circuit, a current may yet be detected,—and the matter is still open for further investigation.

Mr. Spencer detailed the following experiment. He took a piece of sulphuret of copper, and placed it in a narrow glass vessel half filled with sulphate of copper in solution, and immersed the sulphuret about half its length in this solution. He then added common salt, taking care that it should not intermingle with the cupreous solution underneath. He then placed it in the dark, and left it for a week. At the end of that time he examined it, and had the satisfaction of observing that several portions of it had become covered with very minute crystals of the pure metal. At the end of a fortnight still more beautiful crystals had been deposited. This experiment, Mr. Spencer stated, was made for the purpose of corroborating the experiment of Mr. Fox, in which that gentleman took a small portion of sulphuret of copper, to which he attached a piece of copper wire, and at the other end a portion of zinc; the arrangement was separated by a wall of clay, with sulphate of copper in solution in one cell, and a saline solution in the other. Mr. Spencer conceived, that by his arrangement the objection to Mr. Fox's experiment was overcome, viz. that it was always necessary to suppose the presence of two metals.

A communication was read by the Secretary, from Prof. Hess, of St. Petersburgh. It consisted of a description of a modification of Prof. Liebig's apparatus, for the analysis of organic substances, and is particularly useful, according to its inventor, in the decomposition of substances which are susceptible of but slow combustion,—of resins, fatty substances, liquids not very volatile, and particularly of solid bodies. The only difference in the mode of conducting the analysis from that adopted by Liebig is, the attachment at both ends of the tube of combustion of a caustic potash apparatus, and at one end that of a gas holder containing oxygen, upon the principle originally introduced by Dr. Prout, of London. A description of the apparatus has been published in the *Bulletin Scientifique*, of St. Petersburgh, and will be understood at once by the analytic chemist on reference to the figure. The heat is communicated by means of a lamp, described and figured by Professor Hess, in *Liebig's Annalen der Pharmacie*, for 1838.

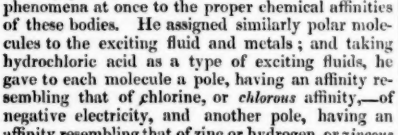
'On a small Voltaic Battery of extraordinary energy,' by W. R. Grove, Esq.—In a letter published in the *Phil. Mag.* for February, I stated (said the author) some reasons for hoping that by changes in the constituents of voltaic combinations of four elements, we might greatly increase their energy. At that period I sought in vain for improvements, which a fair induction convinced me were attainable, but, being in the country, all my experiments were with copper, as a negative metal. I was constantly unable to use concentrated nitric acid as an electrolyte, and its importance never occurred to me until forced upon my notice by an experiment, which I made at Paris for a different object. This was an endeavour to prove the dissolution of gold in nitro-muriatic acid to be an electrical phenomenon, or rather that this (as I believe, with Sir Humphry Davy every other chemical phenomenon,) could be resolved into an electrical one, by operating on masses instead of molecules. The experiment was the following:—The extremities of two strips of gold leaf were immersed, the one in nitric, the other in muriatic acid, contact between the liquids being permitted, but mixture prevented by an interposed porous diaphragm. In this case the gold remained undissolved for an indefinite period, but the circuit being completed by metallic contact, either mediate or immediate, the strip of gold in the muriatic acid was instantly dissolved. Thus, it seems, that the affinity of gold for chlorine is not able alone to decompose muriatic acid; but when it is aided by that of oxygen for hydrogen, the decomposition is effected. The phenomenon bears much analogy to ordinary cases of double decomposition. The two gold strips in the experiment being connected with a galvanometer, occasioned a considerable deflexion; and it now occurred to me, coupling this experiment with my previous observations, that these same liquids, with the substitution of zinc and platinum for the gold leaf, would produce a combination of surpassing energy. My expectations were fully realized; and, on the 15th of April, M. Becquerel presented to the Institute a small battery of my construction, consisting of seven liqueur glasses, containing the bowls of common tobacco-pipes, the metals zinc and platinum, and the electrolytes concentrated nitric and dilute muriatic acids. This little apparatus produced effects of decomposition equal to the most powerful batteries of the old construction. I have since this tried various combinations upon the same principle, and though some of the rarer substances,—such, for instance, as chloric acid,—have produced powerful effects, I have found none superior, and few equal, to the preceding. I have, therefore, directed my attention to using these materials, and rendering the apparatus more economical, although I soon found that it was not so expensive as it at first appeared—the platinum remaining unaltered. Dilute nitric acid diminishes the energy; nitro-sulphuric acid acted as an electrolyte much as nitric acid; it is an excellent conductor, yielding oxygen at the anode, and hydrogen at the cathode. Applying this to my battery, I found it to succeed admirably, and hence a considerable diminution of expense on the side of the zinc, and found salt and water nearly equal to dilute muriatic acid. It also removes the necessity of amalgamating the zinc; but it sometimes, though rarely, disaggregates the porous ware, from the crystallization of nitrate of soda; and, therefore, in large batteries, I should prefer muriatic acid diluted with five times its volume of water. By using flattened parallelopiped shaped vessels, the concentrated acid is much economized, and the metals approximated. Now, according to Prof. Ritchie, the power is inversely as the square root of the distance between the metals. The hastily constructed battery which I have to present, consists of an outer case of wood, height 7$\frac{1}{2}$ inches, breadth 5, width 3, (it should be of glazed earthenware, similar to the Wollaston troughs,) separated into four compartments by glass divisions; in these compartments are placed four flat porous vessels, the interior dimensions of which are 7, 2$\frac{1}{4}$, and 3-10ths of an inch, the thickness of the walls $\frac{1}{8}$ inch; they contain each three measured ounces; the metals, four pairs, expose each a surface of 16 square inches, and the battery gives, by decomposition of acidulated water, 3 cubic inches of mixed gases per minute; charcoal points burn brilliantly, and it heats 6 inches of platinum wire of an inch diameter; its effect upon the magnet, when arranged as a single pair, is proportionally energetic; it is constant for about an hour without any fresh supply of acids. The porous vessels are identical in their constitution with the common tobacco pipe. As far as my experiments go, its power, with reference to the common *constant* battery, is *cæteris paribus* as 6 to 1, but the relative proportions vary somewhat with the series. The cost of the apparatus is 2*l.* 2*s.* During the operation of this battery, the nitric acid, by losing successive portions of oxygen, assumes first a yellow, then a green, then a blue colour, and lastly becomes perfectly aqueous; hydrogen is now evolved from the platina, the energy lowers, and the action becomes inconstant. It is worthy of remark, as an argument for the secondary nature of metallic precipitation by voltaic electricity, that the oxidated or dissolved zinc remains entirely (or at least by far the greater portion) on the zinc side of the diaphragm; the hydrogen alone appears to be transferred; and yet the reversal of affinities which the theory of reduction by nascent hydrogen supposes, is an enigma difficult of solution. I have invariably observed in this battery a current of endosmose from the zinc to the platinum, or with the current of positive electricity.

The rationale of the action of this combination, according to the chemical theory of galvanism, appears to be as follows:—In the common zinc and copper combination, the resulting power is as the affinity of the anion of the electrolyte for zinc, minus its affinity for copper; in the common constant battery it is as the affinity of the anion for zinc, plus that of oxygen for hydrogen, minus that of hydrogen for copper. In the combination in question, the resulting power is as the affinity of the anion for zinc, plus that of oxygen for hydrogen, minus that of oxygen for azote. Nitric acid being much more readily decomposed than sulphate of copper, resistance is lessened and the power increased; and no hydrogen being evolved from the negative metal, there is no precipitation upon it, and consequently no counter-action. I need scarcely add a word as to the importance of improvements of this description in the voltaic battery. This valuable instrument of

chemical research is thus made portable, and, by increased power in diminished space, its adaptation to mechanical, especially to locomotive purposes, becomes more feasible.

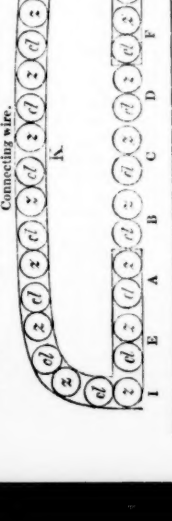
Mr. Spencer then exhibited a cylindrical battery, so as to include great intensity in small space.—Prof. Graham considered Mr. Grove's battery an important improvement, and could only account for its power from the circumstance, as in Daniell's battery, of the removal of the hydrogen.

'On the Theory of the Voltaic Circle,' by the President.—Prof. Graham first explained the views now received of the propagation of electrical induction through the fluid and solid elements of the voltaic circle, by the formation of chains of polar molecules, each of which has a positive and negative side, and in which no circulation of the electricities is supposed, but merely their displacement and separation from each other in the polar molecule. These electricities in the polar molecule of hydrochloric acid, for instance, are displaced, when the acid acts as an exciting fluid, and the positive electricity located in the chlorine atom, and the negative electricity in the hydrogen atom. These electricities are, at the same time, made the depositories of the chemical affinities of the chlorine and hydrogen respectively. Mr. Graham proposed to modify this theory so far as to abandon the idea of electricities being actually possessed by these bodies, and to refer the phenomena at once to the proper chemical affinities of these bodies. He assigned similarly polar molecules to the exciting fluid and metals; and taking hydrochloric acid as a type of exciting fluids, he gave to each molecule a pole, having an affinity resembling that of chlorine, or *chlorous* affinity,—of negative electricity, and another pole, having an affinity resembling that of zinc or hydrogen, or *zincous* affinity, instead of positive electricity. When zinc and acid are in contact, the polar state of a single chain of molecules might be represented as in the figure.

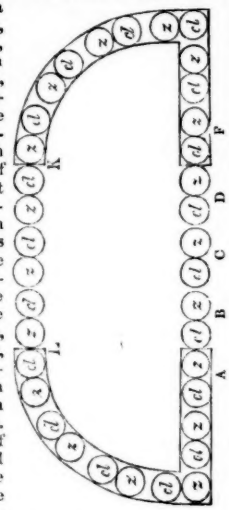


The particle of acid B, next the zinc, has its chlorine atom in contact with the metal and its hydrogen atom distant from it, marked respectively *cl* and *z* in the figure. By induction, the *z* of B causes the *cl* of C to be chlorous, or the molecule of acid C to become polar, and that again the molecule D. While in the zinc, the molecule being supposed to contain two chemical atoms, while the external atom of A becomes zincous, from its contact with the acid, the other atom becomes chlorous; so that these atoms of this molecule may be marked *cl* and *z*, and so also the molecules E and I of the zinc, which become polar by induction.

The second diagram illustrates how this chemico-polar condition is propagated round a voltaic circle, A, E, I being the zinc plate in contact with the acid B, C, D; F, G, H, the copper, connected with the zinc by a connecting wire, H, K, I. The molecules A and B of the zinc and acid being polar by contact, which is sufficient to develope their affinities, an induction through the zinc from A, and through the acid from B, which, it will be observed, by inspection of the diagram, conspire to produce the same polar condition in the molecules. The result is, that the *z* of A is zincous both primarily and by induction, and its affinity for the *cl* of B greatly increased; and consequently, combination can take place between these atoms when circuit is completed, but not otherwise.



If the connecting wire be broken at K, as in the third figure, and a decomposable liquid, such as hydriodic acid, be interposed between the extremities K, L, a chain of polar molecules comes also to be established in that liquid, the iodine (which is the analogue of chlorine) being the seat of the chlorous affinity, and the hydrogen the seat of the zincous affinity. It will be observed, that the extremity K of the wire connected with the copper plate is zincous, or has zincous affinity, and consequently attracts the iodine which appears there, when decomposition occurs. The extremity L of the wire connected with the copper plate is chlorous, or has the affinity of chlorine;



and consequently, the hydrogen of the hydriodic acid, marked *z*, is eliminated there when decomposition occurs. These poles in the decomposing cell of the voltaic circle have, from their importance, always received peculiar appellations, which, with two other terms, Mr. Graham changes as follows:—

Synonymes.

Chlorous ..Negative.
Zincous ...Positive.
Chloroid ..The negative pole, the cathode, the platinode.
Zincoid....The positive pole, the anode, the zincode.

The term chloroid means, what possesses the properties of chlorine, quasi-chlorine; zincoid, what possesses the properties of zinc, quasi-zinc. Mr. Graham afterwards endeavoured to show, that electrolytes were bodies which, like hydrochloric acid, possessed a salt radical and basyle element, which might be the seat of the chlorous and zincous affinities, and which might, indeed, be called the chlorous and zincous elements of the electrolyte; so that the same view was applicable to electrolytes in general.

Section C.—GEOLOGY AND MINERALOGY.

Dr. Buckland drew the attention of the meeting to the subject of fissures in chalk, for which a variety of causes might be assigned, and also mentioned the name by which the appearances, described on the previous day by Mr. Lyell, were known in France, that of *Puits naturel*. He agreed in almost every respect with Mr. Lyell in his mode of accounting for this phenomenon; in particular, he considers Mr. Lyell has well explained the origin of the clay that lines these fissures. The gravel which covers the chalk he conceives to have been accumulated under salt water; and after the elevation of the strata, so as to become dry land, the clay lining was formed by the downward filtration of atmospheric water, carrying with it the material in solution. In all limestones there are small quantities of clay and sand, and he adduced an example from the caves at Lunel, near Montpelier, where an analogous deposit may be seen. The action of acid is not confined to chalk rocks alone, but is exemplified in a remarkable manner upon granite in Auvergne. There, a well may be seen that daily collects carbonic acid from the superior specific gravity of the latter; into this, birds are thrown, to experience the stupifying effects, for the edification of the curious. In Nassau and the neighbourhood of the Eifel many acidulated springs occur; the carbonic acid of which has been supposed, by Von Buch, to have been derived from the igneous rocks below. Dr. Buckland conceives that limestone caverns have been caused by the disturbances experienced by the rocks, and that these caverns have been finished off by the action of acidulated water. Calcareous matter has been thus removed, and insoluble matter left behind, as in the bone cave of Kirkdale, where we find many nodules of chert, often with echini upon them. Dr. Buckland exhibited some drawings of scenery near Syracuse, in Sicily, where there seems to have been an ancient town of houses cut out of the solid rock, in all likelihood inhabited by Troglodytes, before the arrival of the Greek colonists in that island; many of these caves are probably enlarged caverns. The ancients seem to have availed themselves of large masses of stone in executing their great architectural works. In that neighbourhood a temple has been erected, with columns cut out of the solid rock; and four other similar columns still remain, four miles off, never having been removed from the quarry. Also a staircase in the ear of Dionysius has been cut out of a solid block that has fallen down. Near Syracuse is a cave shaped like an inverted *Puits naturel*, the sides of which are rough, evidently so caused by the action of acidulated water. In caves where a dry decomposition is going forward, a fine powder is found, but in this cave there is none.—Dr. Buckland then called the attention of the meeting to the singular phenomena in the chalk near Greenwich, where the surface of this rock, after being bared of the tertiary gravel, is found to present numerous holes in its surface. This appearance of the surface of chalk is by no means uncommon; late excavations near Beaconsfield have exposed to view, not only superficial phenomena, but others, similar to the sand-pipes described by Mr. Lyell. The surface of the chalk appears to have been corroded by some chemical action, which has taken up the soluble material, and left the insoluble matter behind. We find rolled pebbles everywhere on the surface of chalk. In the county of Antrim the flints on the surface have been reddened by the contact of the igneous rock; and a thin red stratum occurs between the chalk and the superincumbent trap. Dr. Buckland showed drawings of the phenomena of the chalk near Greenwich, made by Mr. T. Webster. In that vicinity an advantage is offered the geologist by a horizontal section being afforded him; the chalk having being taken off a considerable space. Geologists agree, that after the formation of chalk deposits considerable disturbance ensued, and owing to volcanic action a vast quantity of carbonic acid was generated, which must have operated in widening the fissures of chalk rocks which had been fractured by these disturbances. Dr. Buckland does not agree with Mr. Lyell in considering these sand pipes as chimneys for the carbonic acid, as he sees no reason why the acid should come up in one place more than another, and a place serving as a chimney should bear the marks of corrosion. Dr. Buckland concluded by instancing the example of fishes killed by carbonic acid by Dr. Daubeny. In these, the death of the fish was very sudden, none living above five minutes. In volcanic districts the carbonic acid generated must have had a similar effect, and many specimens of fossil fish show the animals to have died suddenly, from the perfection of their preservation. He also insisted upon the importance of impressions of foot-marks, of atmospheric and of watery action on the surfaces of rocks, and stated that the most interesting point now in Geology is the examination of those surfaces as they were exposed in different ages.

Mr. Lyell mentioned some new facts, which he had just learned, with regard to the chalk near Norwich. At Higham, sand pipes had been observed, which were not perpendicular, but both slanting and tortuous. Sometimes a covering of chalk marl is found upon their upper extremities, which must have come there after the sinking of the rubbish. At Helseden is a very large pipe, twenty-three feet across; in it are found two indurated masses of white sand. At Thorpe, a pipe is found running up from the chalk into the superincumbent crag, and sand above that, the whole being covered with the usual gravel. The clay lining is found throughout, and over this lining is a thin stratum, with impressions of shells. Near Norwich, many springs come up in the chalk, and sand pipes are sure to be found near them. There is a difficulty in explaining exactly the formation of the larger pipes, especially those that taper, some of which may have served as funnels for the escape of carbonic acid gas; but the smaller cavities were, in all likelihood, formed by water coming downwards, as suggested by Mr. De la Beche on the day before.—Dr. Buckland said, that water lying in holes on chalk would always retain a small quantity of carbonate of lime, and there would be a constant erosion

by atmospheric water falling into these cavities, and then being dried up. Appearances similar to the sand pipes may be seen in any gravel bed after a heavy fall of rain.—Mr. Strickland mentioned that at Henley-on-Thames he knew of three cavities in chalk, and filled with gravel, having originated in the course of seven years. Subsidences in the stratum over the chalk never occur where its surface is denuded, as the water is then absorbed by the chalk.

Mr. De la Beche called the attention of the Meeting to the Geological Map of Cornwall and Devon, which he had made for the Ordnance Survey; and a more beautiful specimen of scientific topography has never been exhibited:—it excited universal admiration.

The Rev. D. Williams then read a paper on the Rocks of South Devon and Cornwall, of which the following is an abstract. He exhibited, at the same time, a map and section of the country. He commenced by some observations on Mr. Weaver's opinions, lately published in the *Philosophical Magazine:* with that gentleman he does not agree, especially on the question of conformity. Mr. Williams then explained the meaning of unconformity and conformity: where the former exists between two series, the upper or younger, and those immediately underlying, or older rocks, do not commonly preserve a parallel angle of inclination with the horizon, or dip—neither do they coincide in their angles with the meridian, or strikes; during any continuous range, they intersect or fly off from each other, terminating more or less abruptly at their junction. Of course, conformity is the converse of this. In the ascending order, from Cunningham Park, near Bridgewater, on the N.E., to the Land's End, in Cornwall, on the S.W., (with the exception of the gneiss and mica slate of Bolt Head and Start Point,) we have one great group of the greywacké system, consisting of nine—perhaps ten—consecutive series, united in one great whole, either by mutual alternations, or by beds of a composite or neutral character. The mountain limestone is of very diminutive dimensions, and generally occurs in hollows, in elliptical patches, the intervals being mostly filled with Coddon Hill grits, or black anthracitous slates. This grit may be traced from its intercalation among the Trilobite slates, until it constitutes the base of those containing plants, and even among the lower members of those slates, so as to form a part of them: indeed, it serves to identify the intermediate mineral horizon, which divides the culm and plant-bearing series into an upper and lower. In the floor above the mountain limestone are comprised, on the north, the Coddon Hill grit, the limestones and slates of Bampton, Hockworthy, &c., and, on the south, the slates, grits, plant-bearing beds, limestones, &c. of South Devon and Cornwall. The region on the east of Dartmoor, from the black limestone at Doddiscombe Leigh, to the mouth of the Dart, is one constant series of alternations of killas, coral-limestones with plants and carbonaceous matter, Coddon Hill grits, &c.; every attendant appearance showing them to be one great consecutive deposit—perhaps derived from different sources—subjected to the same forces, and interchanging levels with each other. To the east of Dartmoor the alternations are, perhaps, more unequivocally defined, while, on the west, the order is the same, but differs in degree. The plea of unconformity is thus abundantly negatived, so that almost all Cornwall and South Devon might be coloured alike in a geological map. These districts have been, probably, a volcanic region during a period of prolonged activity, and the great granite dyke extending from Oakhampton to the Land's End and Scilly, the residuum of the volcanic furnace, may be about the same age as the parallel ranges of the Grampians, and of the north of Ireland. Mr. Williams concluded by expressing his opinion that the entire superstructure of Exmoor had been completed before the deposition of the slates of Cornwall.

This paper was followed by a communication from Mr. Austen, on the Fossil Remains of the Limestones and Slates of South Devon.—He conceives that a great identity of species can be established between the Radiaria, Mollusca, and Crustacea of a portion of the series of the Rhine, and that of South Devon. Both districts present many forms of animal structure, such as in this country we should call carboniferous; and as many as forty species—some hardly, some not at all, distinguishable from those of our mountain limestone. A list of these fossils was read. Mr. Austen considers as the geological equivalents of the slates and limestones of South Devon, those of the Rhine and the Eifel. Of the same age are strata in the south of Ireland; and at Nehou and St. Sauveur in Lower Normandy, many of the same fossils occur. He assigns no particular name to the rocks of these localities, though their place in the system of Northern Europe seems to be a little below our carboniferous series. Old red sandstone is an unfit name to designate the limestones and roofing-slates of South Devon, or the white sandstones of Lower Normandy and Brittany. Local names, generalized, are still worse, as they seem to assume the very prevalent, but questionable notion, that, at various former periods, from the poles to the equator, certain uniform conditions and forms of animal and vegetable life universally prevailed.

Section D.—Zoology and Botany.

Mr. Goodsir read a paper, 'On the Follicular Stage of Dentition in the Ruminants, with some remarks on that process in the other orders of Mammalia.'—Mr. Goodsir commenced by stating, that since the last meeting, he had detected the follicular stage of dentition in the pig, rabbit, cow, and sheep, but that he had not had an opportunity of examining it in those animals in which observations would have been most valuable. He had been able to verify, what at that time he stated as probable, viz.: that all the permanent teeth, with the exception of the first molar, which does not succeed a milk tooth, are developed from the internal surface of cavities of reserve, and that the depending folds of the sacs of composite teeth, are formed by the lips of the follicles advancing inwards, after closure of the latter. He then described the progress of developement of the pulps and sacs of the teeth in the cow and sheep, from their first appearance, as minute as possible, on the full surface of the membrane of the mouth, or on the internal surface of the cavities of reserve, till they have acquired their ultimate configuration. In the course of this description, he announced the fact, that at an early period of the embryonic life of these animals, they possess the germs of canine and superior incisive teeth; the former existing as developed organs in two or three genera only of ruminants, the latter being found in the aberrant family of camels. Mr. Goodsir stated, that these germs presented themselves under the form of slight dimples in the primative groove, and that after the closure of the latter, they remain for a short time opaque nodules imbedded in the gum, in the course of the line of adhesion. The existence of germs of canines and superior incisors in the cow and sheep, is highly interesting, as it shows how general the law of unity of type is within certain limits. Geoffroy St. Hilaire was the first to announce the existence of tooth germs in the fœtus of the *Balæna Mysticetus*, a fact which has been verified by Dr. and Mr. Frederic Knox, in whose museum there is a preparation exhibiting the germs under the form of sacs and pulps. Although the germs never arrive at this stage of perfection in the cow and sheep, they are yet distinct enough to indicate their existence; and the author of this paper has no doubt, that when the embryos of other partially or wholly edentalous mammals have been examined, similar results will be obtained. The author then proceeded to state, that the peculiar manner in which the sac of a ruminant molar, and probably of every other composite tooth, is formed, may be best seen in longitudinal or transverse sections of the sac and pulp of the fourth permanent molar of the sheep or cow. The internal surface of the cavity of reserve is seen to send in a fold or folds; when these meet, they begin to curve towards the papilla, and to enter parallel to one another the cavity or notch which is simultaneously forming in the latter. As soon as the edges of the folds meet, the granular matter denominated enamel pulp by Hunter, (and the formation of which was described by Mr. Goodsir in the human embryo, at the last meeting of the Association,) begins to be deposited, cementing together the opposing folds, sealing up the new sac, separating it from the rest of the cavity of reserve, filling up the space existing between the pulp and sac, and ultimately assisting in the formation of the depending folds of the latter. The author then referred to the distinction which must be drawn between those permanent teeth, which are developed from the primitive, and those which are developed from the secondary groove; and stated that he had been in the habit of dividing the teeth of these animals, the dentition of which he had examined, into three classes, viz.: 1. Milk or primitive teeth, developed in a primitive groove, and deciduous. 2. Transition teeth, developed in a primitive groove, but permanent. 3. Secondary teeth, developed in a secondary groove, and permanent. Mr. Goodsir expressed a hope that other anatomists would verify and extend this line of research, as the results appeared to him not only confirmatory of certain great general laws of organization, but as leading in his opinion, by the only legitimate path, to the determination of the organic system to which the teeth belong, (a subject exciting great interest at present,) and as it might enable us, in investigating the relations of dental tissue to true bone, to avoid the error of confounding what there appears to be a tendency to do, analogy with affinity. The paper concluded with a recapitulation of the principal facts contained in it. 1. In all the mammalia examined, the follicular stage of dentition was observed. 2. The pulps and sacs of all the permanent teeth of the cow and sheep, with the exception of the fourth molar, are formed from the minor surfaces of cavities of reserve. 3. The depending folds of the sacs of composite teeth, are formed by the folding in of the edges of the follicle towards the base of the contained pulp, the granular body assisting in the formation of these folds. 4. The cow and sheep (and probably all the other ruminants,) possess the germs of canines and superior incisives, at an early period of their embryonic existence.

'On the Preparation of Fish,' by Mr. Wilde.—After some observations on the methods at present in general use, Mr. Wilde observed,—My mode of preparation is as follows: I make an incision through the scales to the muscles, commencing about where the operculum joins the cranium, and continue it parallel with the dorsal outline to the centre of the tail. A similar cut is made from above the pectoral fin, till it, also, meets in the centre of the tail; by this means, somewhat less than a third of the side is included between the lines. The fish is kept steady on a smooth board, adhering to it by the natural gluten, water being poured over it from time to time, so as never to allow the scales to dry. The skin is then dissected back as far as the dorsal margin, where it meets the bony rays which support the fins. These are cut across, as close to the skin as possible, with a strong pair of scissars or a cutting forceps. A similar process is used towards the abdomen, taking care to keep as close to the facia to which the scales are attached as possible. The first vertebra is then divided from the cranium, and the skinning process continued by lifting up the body and leaving the skin adherent to the board, from which it should never be removed, if possible, till the dissection is completed. Difficulty will be experienced towards the tail, where the muscles become more tendinous, and are attached to the subcutaneous facia. The rays of the caudal fin are then divided from the vertebra, and the body removed entire. The gills are next taken out, and the flesh of the cheeks and any remaining portion about the head or thorax. It is as well, perhaps, to leave in the scapulæ, or a large portion of them. An opening is made into the side of the cranium, where it will be found very thin, and the brain taken out. The eye is completely removed on the reverse side; a hook, passed down through the orbit, transfixes the back of the sclerotic of the other eye, in which an opening is made; the finger then pressed on the cornea in front will squeeze out the lens and humours, retaining the iris perfect in its place, and I have lately succeeded in retaining the gills, if necessary; the tongue is left in, and the fish is then cleansed from all impurities, taking care not to stretch the skin nor to injure the scales. It is next dried, and either well anointed with arsenical preparation or the spirituous solution of corrosive sublimate. The eye is filled with cotton from the opening in the back—care being taken to keep the iris in its natural position. The cranium is also stuffed, and flakes of tow, cotton, or any material of light description, laid along the body till you consider a sufficiency to give the form of the animal is put in. The reflected edges are then returned—the fish removed from the

board, and placed with the front up—the tail and fins, expanded, are pinned down in their natural position, on cards, supported by little bits of cork,—the fish is given its proper shape, and the inequalities on its surface, smoothened off with a soft brush,—it is then set to dry in a current of cool air, with little sun, and should be watched to see that it dries equally and that no part of the skin shrinks more than another. If it should, a brush, wetted in cold water, touched upon the part, will restore it. It should be varnished the moment it is sufficiently dry, and the cards, &c. removed from the fins, which will now retain their natural position. I have tried several varnishes, and found the common copal, diluted with turpentine, the best. The cornea now becomes hard, transparent, and continuous with the surrounding skin—the wadding may be removed through the back of the sclerotic, and a bit of foil introduced in its place, of the colour originally possessed by the animal, in many of which we know the tapetum is very brilliant. Finally, a pin at head and tail will retain the preparation on a board, from which it stands out in bold relief, and preserves its shape and colours better than any other I have yet seen. Much difficulty will be found in skinning those on which the scales are very small, as in the mackerel tribe, and the thin skin will not, of itself, preserve the contour of the fish; in those cases, I found that pasting a few layers of common brown paper on the inside of the skin, until it acquired sufficient dryness to retain the position with the stuffing underneath, answered the purpose perfectly. I find this method is not so applicable to the shark and eel tribes; the most effectual mode of preserving which will be by drawing the body through the mouth. Myriads of small red ants swarm in Madeira. These I found so destructive to entomological specimens, that a whole trayful would be eaten up in a night; yet none of my fish suffered from them. Specimens properly prepared can easily be brought home by being pinned lightly on the boards, and placed in boxes with an interval of a few inches between each. When dry, they do not create the slightest smell, and when brought home they can be afterwards damped, reset, and the position altered by removing the varnish with a little turpentine and damping the interior. Should not a second specimen be at hand, or good plates to be procured, I have been in the habit of drawing the outline of the fish upon a board before I commenced the dissection.

Mr. Strickland observed, that he thought arsenical soap was a bad preparation for animals; he wished it had never been used in his specimens, as it produced in them an early decay.—Dr. Burchell stated, that in specimens he had brought from Africa, the arsenical soap had been used, and they had kept well for twenty-five years.—Mr. Gray observed, that the plan of drying fishes' heads with the gills had been adopted by Dr. Riley with success.—Mr. Wilde stated, the best plan of retaining the outline of the fish, was to lay the specimen on a board and to chalk the outline, and afterwards, when dried, to make it assume the original outline by comparison with the chalked board.

Mr. R. Patterson exhibited drawings made from living specimens of a species of Ciliograde, taken in July last at Bangor, County Down. Its occurrence on the Irish coast had first been announced in a note appended to his paper on the *Cydippe pomiformis*, published in the Transactions of the Irish Academy. This animal he had referred, though with some doubt, to the genus Bolina, of Mertens, and named it provisionally Bolina Hibernica. He mentioned some particulars relative to its appearance, movements, frangibility, and luminosity, and stated that his object in exhibiting the drawings was rather to ask for, than to impart information; and he wished to know whether it had been observed in any other locality, and to what extent the power of luminosity was possessed by other British Ciliogrades.

Prof. Jones thought that the luminosity of marine animals could not be identified with particular species, nor was it confined to the living animals, as they gave it out equally when dead. He would inquire of Mr. Forbes if he could confirm the observations of Dr. Grant on the nervous system of *Beroe pileus*. He had not been able to detect a nervous system, but believed that delicate vessels existed in these animals.—Mr. Forbes stated, that he formerly thought he had confirmed Dr. Grant's observations, but subsequent inquiry had convinced him that he was in error. Difficulty existed in examining these animals, but he had found an easy plan to do this, by placing the animals in a watch glass. He had found Acalephæ in a state in which they gave no indications of luminosity, but on being agitated they were immediately luminous. Dr. Macartney had described a Beroe that was constantly luminous. He believed, however, with Ehrenberg, that this depended on the condition of the ovaries, and only occurred at certain periods.—Mr. Goodsir gave some account of an Alcynöe, which, whenever it was touched, gave out gleams of light, and these proceeded from the circular ridges of ciliæ.

'On the Ciliograda of the British Seas,' by Edward Forbes and John Goodsir.—The Ciliograda of the British seas belong to three genera, Cydippe (Eschscholtz), Alcynöe (Rang), and Beroe (Linnæus). In the first of these genera, the animal is provided with long semi-pinnated retractile filaments or tentacula, contained in cavities which open towards the posterior extremity of the animal. The Cydippe swims with its mouth upwards. In the second genus, Alcynöe, the mouth is uppermost, and is provided with two large natatory lobes or lateral expansions, and with four compressed tentacula ciliated at their edges. The third genus, Beroe, also swims with its mouth downwards, but has neither natatory lobes, filamentary appendages, or oral tentacula. There are four species of Cydippe inhabiting the British seas: one, *C. pileus*, described by Linnæus, *Cydippe Flemingii*, (probably the *Beroe ovatus* of Fleming,) described by Mr. Forbes in the 16th number of the Annals of Natural History. *Cydippe pomiformis*, described by Mr. Patterson in the Transactions of the Royal Irish Academy; and a new species, which it is proposed to name *Cydippe lagena*. Of the genus Alcynöe, we have observed two British species. One species only of this genus has hitherto been observed, the *Alcynöe vermiculata*, discovered by Rang on the coast of Brazil. Of the genus Beroe, we have discovered the *Beroe cucumis* of Fabricius in the British seas. To this genus also, appertains the *Beroe fulgens* of Macartney. The British species may be summed up as follows:—

CILIOGRADA.

Genus *Cydippe*—Filamentary appendages.
Genus *Alcynöe*—Tentacula round mouth.
Genus *Beroe*—No tentacula or appendages.

1. *Cydippe pileus*, (Linnæus)—Rows of cilia, 19 or 20, on the summits of the lobes—filamentary appendages white. St. Andrews. Mouth of the Thames, (Dr. Grant).
2. *Cydippe Flemingii*, (Forbes)—(an *Beroe ovatus*, Fleming?)—Rows of cilia 36, on the summits of the lobes—filamentary appendages white. St. Andrews.
3. *Cydippe lagena*, (Forbes)—Rows of cilia about 25, placed in the furrows of the lobes—filaments white. Coast of Ireland.
4. *Cydippe pomiformis*, (Patterson)—Rows of cilia about 20—filaments rufous. Coast of Ire and and Mouth of Forth.
5. *Alcynöe rotunda*, (Forbes and Goodsir)—Ovate, rounded, crystalline—tentacula rounded at their extremities—natatory lobes forming half the animal. Kirkwall Bay, Orkney.
6. *Alcynöe Smithii*, (Forbes)—Elongato-pyriform, subcompressed, crystalline—natatory lobes not more than a third of the whole length—tentacula acute.
7. *Beroe cucumis*, (Otho Fabricius)—No spots on external surface, internal dotted with red points—ciliferous ridges red. Isle of May at the Mouth of Forth.
8. *Beroe fulgens*, (Macartney).

Mr. Forbes added, that the red ovaries described by Dr. Grant, as imbedded in the substance of *Cydippe pileus*, seem to have originated in error, he having found the same appearance in many specimens to arise from the circumstance of the animals having swallowed certain little crustacea with very large red eyes, on which they feed. The authors of this paper have observed the true eggs of Cydippe, which first appear in the ciliferous ridges, and pass along certain vessels to the stomach, whence they would appear to be ejected by the mouth. These eggs are transposed, and under the microscope appear as if filled with globules. When the animal is in egg, the ciliferous ridges and vessels are milky, an appearance first noticed by Mr. Patterson, of Belfast.

Mr. Gray said, that it was to be regretted, that so little had been done towards our knowledge of the Medusæ and related tribes. The best work on this subject was that of Eschscholtz, but this extended only to the animals of the Pacific Ocean.—Mr. Lankester thought, that much was still wanting in our knowledge of the British marine invertebrata; and, in fact, the contributions of Mr. Forbes proved this. He had been lately struck with a passage in Chamisso's account of the voyage of Kotzebue, in which he states, that having landed at Plymouth before starting, he found on the coast there many annelides and other Invertebrata, of which he could find no account in the works which he possessed.—Mr. Patterson explained and illustrated a singular structure, observed by him in the tentacula of some small Medusæ. These organs, when disturbed, exhibited a number of oblong bodies united by a colourless and elastic substance, and these, when examined under a high magnifying glass, were found to be divided diagonally into two flattened and somewhat pyriform bodies, showing in parts bright crimson and purple tints; and in others, areas perfectly free from colour. The number of these in each tentaculum was about 100, which, when divided into two parts, gave about 800 of these complex structures, attached to an animal of only four or five lines in length.—Mr. Forbes stated, that the contraction of the tentacula and their filaments in the Acalephæ, was generally like that of Cydippe, in which not only the tentacula, but the filaments which covered them, were shortened by the process of curling, each filament forming itself into a closely compacted spinal.—The subject of the luminosity of of these animals was again discussed. Dr. Daubeny observed, that there were two kinds of luminosity; first, a dark diffuse light; and secondly, a bright light, resulting from the agitation of the waters.—Mr. R. Taylor stated it to be the opinion of Ehrenberg, that it depended on electrical action, and conceived that it might depend on the exercise of volition in the animal.—Prof. Jones thought the points referred to as eyes in the Medusæ, were not correctly named. He thought it impossible, that there should be eyes without a nervous system, to receive the impressions of these organs.—Mr. Gray observed, that eyes might exist, and yet be of little use—as in the common snail; besides that a nervous system might yet be discovered in Medusæ.—Mr. Goodsir had examined the eyes of *Medusa aurita* very carefully. They consisted of peduncles, the base of which was composed of a soft gelatinous mass, like the body of the animal, but the point was studded with numerous red particles, which gave them their red colour. In *Asterias oculata*, the eye consisted of several cavities, which, if covered over by a refracting medium, would constitute an organ very similar to the eye of Crustacea, and other animals higher in the scale of being.

A paper was read by Mr. Wilde, 'On some new Species of Entozoa, discovered by Dr. Bellingham.'

'On the Acceleration of the Growth of Wheat,' by G. Webb Hall.—The object of Mr. Hall's communication was, to call the attention of the meeting to a statement of facts connected with the acceleration of the growth of wheat, and a consequent diminution of the period required for its occupation of the ground, and to exhibit the results of the proceeding, and the benefit deducible therefrom. The ordinary period of growth allotted to the wheat plant may be taken from the middle of October to the middle of August—a period of ten months—twelve, or even thirteen, being not uncommon—while for the ordinary winter wheat, from December to August may be taken as the shortest period of growth: close observation to the progress of the plant, under different circumstances, and a peculiar selection of seed and soil, have reduced this plant to nearly five months. An abundant crop of wheat, which was sown on the 2nd of March, was ready for the sickle on the 15th of August following. This is not a solitary case, nor is it the result of a peculiar season. In the year 1835, wheat sown on the 5th of March was reaped on the 12th of August; and on a previous occasion, wheat sown the 9th of March was reaped on the 11th of August, the produce being forty bushels per acre. A deep tenacious soil is most congenial to the growth of wheat; such soils, however, form a very minute portion of the land of England; to the lighter and more siliceous soils Mr. Hall's observations apply. When wheat is placed upon the lighter soils, its growth and security are alike promoted by artificial pressure and compacting of such soils, which, also, by the addition of manure, acquire a warm and stimulating character; but they as assuredly become quickly exhausted, and therefore the acceleration of the growth and ripening of the plants committed to a light soil, and a diminution of the time required for

perfecting its crops, is congenial to its character, and tends to economize and prolong its productive powers. What he wished to direct the attention of botanists to was, the practicability of so adapting the seed to the soil, as that an early ripe crop should be always obtained, and thus avoid the accidents to which a growing crop was exposed in the depth of winter; besides that, whatever saved time, saved money. A quick crop might be produced by pressing and compacting the soil; and in light soils, well manured, a quicker growth was ensured. In obtaining early crops, two evils must be avoided—the accelerating the process by too great stimulation, and the maturation of the crop at a too recent period. Science would suggest for each case a remedy. One great means of obtaining early crops would be, the use of seed produced by plants that were themselves of early growth.

Dr. Daubeny was glad to find this Section taking up the subject of Agriculture. He thought the rapid growth of plants might depend on the quantity of nitrate of potass, that is found in great abundance in all kinds of manure.—Mr. Babington referred those interested in the subject to Colonel Le Couteur's work, in which he described forty varieties of wheat. —Mr. Lankester observed, that the principle of producing plants of a peculiar kind from seeds of plants bearing that peculiarity, was acted upon extensively in horticulture, and thought the same principle applicable to agriculture. By attention to the laws of physics and organic life, the agriculturist would be able to overcome many of the natural impediments to the fertility of the land.

Section E.—MEDICAL SCIENCE.

Mr. Middlemore detailed a case in which the operation for artificial pupil was performed with success, and presented the patient to the Section for examination. About three years ago much injury was done to the face from an explosion of gunpowder. After recovery of the other parts, the eyes were found to be in the following condition: the right was completely collapsed, the left was staphylomatous, the lens adhering to the staphyloma, but transparent; the lower half of the cornea was opaque, the upper half transparent, but vision destroyed, from the closed iris being opposite to the transparent portion of the cornea. The first effort was to remove the staphyloma, which was done by repeated puncturing of it with a fine needle. When the process of removal was so far completed, as to permit the operation for artificial pupil, the iris was drawn through a small section of the cornea: it bled freely; but on the subsidence of the hæmorrhage and irritation, a sufficient and well-defined opening was found in the iris opposite the transparent portion of the cornea. The external portion of the iris was allowed to remain strangulated by the incision. The patient has already in a great degree recovered his sight, so as even to distinguish large print. He is still under treatment.

Dr. Foville (of Paris) presented a paper, detailing the results of his researches on the Anatomy of the Brain. He commenced by urging the advantages of examining the structure of the brain by manual separation rather than by section, and gave credit to our countryman Willis, as being the first advocate of this method. He showed that the spinal marrow consists of two lateral portions, united by two commissures, between which, on the median line, there exists a double layer of white matter, analogous to the ventricle of the septum lucidum. He pointed out a remarkable difference of structure in the lateral parts of the spinal marrow, between the roots of the nerves, which is rendered most evident by maceration in water, after previous maceration in spirit. He next described the medulla oblongata. Tracing the crura cerebri to the brain, he showed them to consist of two parts,—the one going to the thalamus opticus, the other to the corpus striatum, where they constitute the white matter,—passing through the middle of those bodies, at the upper and outer limits of which they divide into three layers,—the superior, passing upwards and inwards, meets its fellows on the median line, and forms the corpus callosum; the second, or middle, is expanded in the hemispheres, which it constitutes, by lining the cineritious matter of the convolutions; the third, or inferior, and by far the smallest layer, passes to the outer side of the thalamus and corpus striatum, meets its fellow inferiorly, and, ascending with it, forms the septum lucidum. In addition to these facts, he stated his more recent discovery, of several nearly circular systems of white fibres connecting the expansions of the superior part of the crus cerebri, which, from their connexion with the olfactory and optic nerves, and also with the posterior part of the spinal marrow, appear to be essentially devoted to sensation. He also stated his fully confirmed observations, that the pathological affections of the thalamus influence the movements of the opposite side of the body, as those of the corpus striatum do those of the lower extremity. He noticed a similar connexion between the lesions of the cornu ammonis and the motions of the tongue. He combated the idea, that the frontal, parietal, and occipital protuberances, are dependent on special developement of the corresponding parts of the brain, but are rather to be attributed to the distension of corresponding parts of the ventricles. After the reading of the paper, Dr. Foville demonstrated the leading facts alluded to, on the recent brain.

Prof. Macartney said, that his own researches confirmed those connexions of the fibres of the brain pointed out by Dr. Foville; but that a more extensive and minute connexion between the different portions could be traced than those now demonstrated. He also stated, that the roots of the spinal nerves, by a beautiful plexus or net-work prolonged on the spinal column, formed its outer wall, and communicated with those spinal nerves above and below.—Dr. Foville was aware of those more minute communications alluded to; but on the present occasion he did not attempt to give a complete exposition of the nervous system, as being unsuitable, from its great extent.—Dr. Evans stated, that having seen Dr. Foville's pathological statements, as derived from his anatomical investigations in the 'Dictionnaire de Médecine,' he had minutely examined the cases reported by Lallemand, Rostan, and Andral, and found them to corroborate those views in almost every instance.

Prof. Macartney read a paper 'On the Means of repressing Hæmorrhage from Arteries.' He stated that the barbarities practised by the older surgeons, such as burning and searing, instead of the ligature, proceeded from the adoption of the false theory, that a certain amount of inflammation was necessary to the healing process, and, although modern surgeons are easier satisfied, the theory has been preserved; the continental surgeons, however, insisting on a greater degree of inflammation than the British. Even the ordinary ligature, he observed, has been known to fail, from the injury and consequent inflammation inflicted by it; to obviate this, he was induced to try the effects of metallic ligatures, from observing that such substances frequently remained in the body without exciting any uneasiness. On applying ligatures of leaden wire to the arteries of dogs, he found, after death, that they remained *in situ*, without surrounding inflammation, or were removed by interstitial absorption, the arteries being impervious. The same results were observed when the experiment was made on the jugular veins of rabbits. An improvement was made by Mr. Weiss on the leaden ligature, by substituting soft metal wire, capable of being knotted. He next alluded to the well-known fact of the closure of arteries in lacerated wounds; this he attributed to the rupture of the elastic coat and the elongation of the outer or cellular, so as to present merely a minute orifice, which was closed by coagulated blood, and not by the retraction of the artery, as Mr. Abernethy had supposed. In the treatment of stumps, he thought it a matter of doubt, what arteries must necessarily be tied, when the powers which nature possesses to repress hæmorrhage be considered, and the cut surfaces be treated as an open wound, with cold applications. He related a case communicated to him lately by Mr. Darley, near Bray, in Ireland, in which, after amputation of the hand of a child, the stump was dressed with lint, kept wet with cold water, *and no ligature was applied or required*. This, the Professor deemed to be the first case on record, in which amputation was performed without the application of a ligature; he related another case, of severe wound of the thigh, in which the femoral artery was opened, and after some delay was tied, yet hæmorrhage to a considerable amount recurred; nevertheless, by keeping the wound exposed, and cold (in the form of ice) applied, the bleeding was repressed, and the closure of the wound proceeded to a favourable termination.

Prof. Gibson, of Philadelphia, stated, that twenty-five years ago, Dr. Physic had introduced ligatures of leaden wire, influenced by the same considerations as Dr. Macartney. This he wished to state, not as detracting from the merit or originality of the learned Professor's views, but as corroborating them.—Mr. Hodgson agreed with Dr. Macartney, that inflammation was not necessary to the reparation of injuries, but he did not think that what Dr. Macartney called the modelling process, was the usual means nature adopted for the closing of arteries; but contraction. He thought metallic ligatures might prove injurious, from causing ulceration and sloughing of arteries in their process through the body. He also objected to soluble ligatures, such as catgut, as liable to premature softening. Silk ligatures he thought injurious from their too long detention on the artery. Hemp, as in common twine, he deemed the best. He agreed with Dr. Macartney that the exposure of an artery promoted its contraction, but thought it might prove a dangerous experiment: experiments on healthy animals might lead to inferences not to be applied without danger, to seemingly analogous cases of invalid patients.—Prof. Macartney explained, that he did not advocate the metallic ligature in all cases. —Mr. Wickenden detailed a case strongly supporting Dr. Macartney's opinions, as he thought. It was one in which amputation was performed on a patient, whose arteries were extensively ossified: attempts to fix ligatures repeatedly failed, yet by attending to the elevation of the stump, and the application of cold, serious hæmorrhage was prevented.

Dr. Blakiston read a paper 'On the Sounds produced in respiration, and on the Voice.' He commenced by showing that the respiratory sound, coarse and intense when heard in the trachea, became weaker and softer as it approached the periphery of the chest, at which point the sound, during expiration, had almost totally disappeared. The air, in passing along the trachea and bronchial tubes, would meet with solid obstacles, and therefore be thrown into sonorous vibrations at every alteration of direction. The divergence of sound caused by the great subdivisions of those tubes, and the constant diminution in their calibre, would necessarily tend to soften and weaken the respiratory sounds from the trachea towards the air vesicles. But the sounds produced by inspiration were carried up to the ear, placed on the chest, by the current of air during that act; while that produced by expiration, was carried quite in a contrary direction: hence the difference in intensity. It was next shown, that bronchial respiration, occasioned by solidification of a portion of the lung, did not take place in the tubes leading solely to that portion, as had been supposed by Andral and Laennec, but that it took place in tubes leading to healthy expansible vesicles; and the ear being brought into contact with these tubes, perceived the coarse sound of the air passing and repassing in them. It was contended that no sensible part of the sound of vesicular respiration was produced in or around the vesicles, or by the rubbing of the pleura, otherwise it would be heard in expiration; nor in the mouth or fauces, otherwise stertorous breathing would increase its intensity, which it never does. The voice being an instrument of the membranous reed kind, Dr. Blakiston detailed a number of experiments which he had made with different kinds of pipes on the wind-chest of an organ, which led him to conclude that the quality of tone of wind instruments became uniformly more coarse and buzzing in proportion to the strength of the blast, and the thickness and elasticity of their sides; in other words, in proportion as the instrument itself entered into strong vibration. Some instances of the manner in which interference and jarring was produced between those solid vibrations of the instrument and those of the air contained in it, were then given. It was shown that both kinds of vibration were concerned in the formation of the voice; hence, when heard over the larynx, it was found to be coarse and intense; in proportion, however, as these vibrations travelled downwards towards the air vesicles, they were deadened, the aërial waves by the opposing current of expiration, and the solid ones, by the increasing mass of the non-homogeneous mass of the lungs; and at their periphery, no reso-

nance of the voice could be detected. When, however, a portion of the lung became solidified, the current of expiration leading from it was stopped, and the spongy lung was transformed into a more homogeneous, and, therefore, a better conducting substance; hence the voice resounded strongly, and its quality became sometimes so coarse as to produce a tingling sensation in the ear.

Section F.—STATISTICS.

Mr. Langton, on the part of the Manchester Statistical Society, read a report on the educational condition of the county of Rutland. He stated that the Society having previously examined the manufacturing districts where the population was dense, and the rate of increase rapid, had resolved to investigate an agricultural district where the population is scattered and nearly stationary. In comparing the counties of Rutland and Lancaster, the smallness of the parishes in the former appears striking, there being a parish church for every 400 inhabitants; the Roman Catholic population is very small; there is no place of worship connected with that sect in the county: 2-7ths of the population belong to various sects of Protestant dissenters, the Wesleyan Methodists preponderating,—the remaining 5-7ths belong to the Established Church. The population of Rutlandshire was, in 1811, 16,383; in the decennial period between 1811 and 1821, it increased 13 per cent., but in the next decennial period the increase was only 5 per cent. In 1831 the population was—

Males.....	9,721
Females.....	9,664
Total	19,385

Taking the scholars of all ages, 1,119, or about 5 per cent. of population, attend evening and day schools only:—

1,920, or 9.6 per cent. attend day and Sunday schools.
1,276, or 6 per cent. attend Sunday schools only.

Comparing these numbers with those derived from former investigations, the following are the results:—

Sunday Schools.

Manchester and Salford	17 per cent.
Rutlandshire	16
York	12
Liverpool	6

Day and Evening Schools.

York	17 per cent.
Rutlandshire	15
Liverpool	13
Manchester and Salford	10

There are as many endowed or charity schools in Rutland as there are parishes. The teachers are generally of irreproachable character; and the dame-schools, in quiet, cleanliness, and orderly habits, afforded a very gratifying contrast to the schools of the same class in Manchester and Liverpool. Industrial education was very limited, but the girls were generally found sewing or knitting, and in many schools the boys learned to knit. The attendance of pupils is very irregular, as they are frequently detained to assist in farm labour at seed time and harvest. Out of 53 parishes, 46 have Sunday schools: the teachers are generally paid, and are most frequently masters and mistresses of day-schools. There is, however, a great want of systematic visitation. Good school-books are much wanting, and though the teachers are generally moral and respectable, they are not so systematically trained as to be fit to superintend education.—Mr. Slaney called the attention of the Section to the necessity of providing school-books and play-grounds.—Dr. W. C. Taylor stated that he considered the physical education of those who had to earn their bread by manual labour even more important than literary education.—Mr. W. Gregg mentioned several advantages which had arisen from the mere fact of instituting educational inquiries.—Mr. Slaney inquired whether the Report on the State of Education in Birmingham would be brought before the Section; and if not, why it was withheld?—The Rev. Mr. Dunn stated that the census had been taken at a time when the attendance at school was diminished in consequence of commercial distress.—Mr. White, and several other gentlemen, were of opinion that this circumstance gave the report a special value, as it would throw some light on the connexion between mental destitution and physical suffering.—Messrs. Dunn and Saunders said that the Report in its present state was inaccurate, and promised that a more perfect census should be taken. The discussion was protracted, but the decision of the Birmingham committee, though only carried by the casting vote of the chairman, was maintained.

Mr. Rawson then read a very elaborate paper on the Criminal Statistics of England and Wales, the report of which we must defer till next week.

List of New Books.—Narrative of the Voyage of the Beagle, by Captains Fitzroy and King, 3 vols. 8vo. 2*l.* 18*s.*—Maxwell's Victories of the British Armies, with plates, 2 vols. 8vo. 28*s.* cl.—Morton, of Morton's Hope, an Autobiography, 3 vols. post 8vo. 1*l.* 11*s.* 6*d.*—Carlyle on the French Revolution, 2nd edit. 3 vols. 12mo. cl. 25*s.*—Spenser's Works, 5 vols. 12mo. cl., being Vols. XXXIX. to XLIII. of the Aldine Poets, 25*s.*—Hemans's Life and Works, Vol. IV. 12mo. cl. 5*s.*—Cogswell's Sermons, 8vo. cl. 12*s.*—Scott's Theological Works, new edit. fc. cl. 8*s.*—Sermons on Romanism, Preached at Liverpool, 12mo. cl. 7*s.* 6*d.*—Richter's Death of an Angel, translated by Kennedy, post 8vo. cl. 9*s.*—Miriam, a Jewish Tale, 6th edit. 12mo. cl. 6*s.*—The Rose Unique, or Errors Expiated, fc. cl. 4*s.*—Bateman's Sermons, Preached in India, 12mo. cl. 5*s.*—Line upon Line, Part I. 3rd edit. 18mo. cl. 2*s.* 6*d.*—Wilkinson's Public Funds, 12mo. 12*s.*—Chief Justice Marshall's Writings, 8vo. cl. 28*s.*—Johnson on Bills of Exchange, 2nd edit. including the New Act, 12mo. 7*s.*—Millington's Civil Engineering, 8vo. cl. 28*s.*—Grove's Greek and English Lexicon, 7th edit. 8vo. roan, 15*s.*—Vergani's Racconti Istorici, by G. Guazzaroni, 12mo. enlarged, 4*s.* 6*d.* cl.—Cory's Treatise on Accounts, 2nd edit. 8vo. bds. 9*s.* 6*d.*—Leigh's New Road Book of Scotland, 18mo. bd. 9*s.*—Tournaments, or the Days of Chivalry, 32mo. cl. 3*s.* 6*d.*—Colburn's Modern Novelist, Vol. XV. 'Sayings and Doings,' 3rd series, fc. cl. 6*s.*—Lardner's Cyclopædia, Vol. CXVII. 'Literary and Scientific Men of France, Vol. II.' 6*s.* cl.—Lockhart's Scott, Vol. VI. 12mo. cl. 5*s.*—Johnson's Highwaymen, 2nd edit. 8vo. cl. 9*s.*—Bingley's Tales about Birds, square 16mo. cl. 4*s.*—Bennet's Royal Armorie of Great Britain, fc. cl. 10*s.*—Woman's Mission, 3rd edit. 12mo. cl. 3*s.* 6*d.*—The Poultry Yard, by Peter Boswell, 18mo. cl. 2*s.* 6*d.*—Christison's New Agricultural Tables, 12mo. cl. 2*s.*—The Hand-Book of Electricity, 32mo. swd. 1*s.*—Hope's Practical Chemist's Pocket Guide, 32mo. cl. 1*s.* 6*d.*

OUR WEEKLY GOSSIP.

We this week pay our annual tribute to the British Association, and surrender our journal exclusively to its use. Fortunately, these Meetings take place at a time when the Societies, Theatres, Opera, Concerts, Exhibitions, are all closed,—when publishers are weary, and at rest, or dreaming only of future speculations;—so that we have nothing to gossip about, and no novelties of interest on our table. Three or four double numbers, therefore, will enable us to present to our readers a full history of proceedings, with abstracts of many valuable papers, which would otherwise, perhaps, be lost sight of, and, in a few years, forgotten, without trenching greatly on the space usually allotted to matters of a more popular character, though certainly of less permanent interest.—At the adjourned meeting of the Committee, on Thursday, it was decided that the next meeting of the Association should be held at Glasgow, and the Marquis of Bredalbane was elected President.

NOW OPEN.
DIORAMA, REGENT'S PARK.
NEW EXHIBITION.—THE CORONATION of HER MAJESTY QUEEN VICTORIA, in Westminster Abbey, and the INTERIOR of the CHURCH of SANTA CROCE, at Florence, with all the effects of Light and Shade from Noon till Midnight. Both Paintings are by Le Chevalier Bouton.—Open from Ten till Five.

To Correspondents.—We sincerely sympathize with Mr. Jackson, but cannot insert his letter. We were attacked as Journalists, and our readers, therefore, might fairly be presumed to take some interest in the question—but his difference with Mr. Chatto is altogether personal.

The Reports of the preceding Meetings of the British Association may still be had. Those for Dublin are contained in the Monthly Parts for August and September, 1835; Bristol, in August and September, 1836; Liverpool, September and October, 1837; Newcastle, August and September, 1838.

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This day is published,
THE ECLECTIC REVIEW for September.
Contents.
1. The present state of the Catholic Controversy.
2. The Life and Times of Archbishop Sharp.
3. London Exhibitions.
4. Buxton on the African Slave-trade.
5. The Political Duty of Christians.
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15th August, 1839. Secretary to the Council.
The Rev. Mr. Cook, 4, Gower-street North, Mr. Hardy, 38, Mornington-crescent, Hampstead-road, Mr. Haselwood, 20, Upper Gower-street, and Mr. Behan, 16, Euston-square, receive Boarders.
The Lectures in the Classes of the Faculty of Medicine commence on the 1st October; those of the Faculty of Arts on the 15th October.

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